THE QUARTERLY JOURNAL OF SCIENCE, LITERATURE, AND ART.

JULY TO DECEMBER, 1827.

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TO OUR READERS AND CORRESPONDENTS.

The pages of this Journal are impartially open to all communications upon the subjects of Science, Scientific Literature, and the Arts: it is requested they may be forwarded to the Editor one month previous to the publication of each number.

We shall be happy to receive papers from Provincial Scientific Societies, and to publish them either on the part of the Society, or of their respective authors.

Papers deemed unfit for this publication, will be immediately returned to the source whence we received them, with our reasons for their return.

The letters signed B. and T. R. S. we have thought it prudent to suppress for the present.

Several books have reached us for notice in this Journal; but unless they are sent earlier in the Quarter, we cannot insure attention to them.

We have been favoured with communications from Mr. Swainson, Dr. Littledale, Mr. Rose, and E. Z., which we are obliged to postpone.

A letter from a "Member of the Zoological Society," reached us too late for the purpose it was intended to answer. We fear we shall not agree with him in opinion, but perhaps his *second communication* may clear up the difference.

We presume that "A Mechanic" will find the information he requires in Mr. Farey's account of the Steam Engine.

"An old Subscriber" is much in error—the *proceedings* he alludes to are copiously given in contemporary monthly publications; if therefore we followed his advice, our information would be stale. The motives he alludes to are out of the question.

We cannot give "A Vapourer" any authentic information respecting the Steam Carriage, nor do we hear that the Gas Engine has advanced.

ROYAL INSTITUTION OF GREAT BRITAIN,

Albemarle Street, *December* 3, 1827.

A COURSE OF SIX ELEMENTARY LECTURES ON CHEMISTRY, adapted to a Juvenile Audience, will be delivered during the Christmas Recess, by MICHAEL FARADAY, F.R.S., Corr. Mem. Roy. Acad. Sciences, Paris; Director of the Laboratory, &c. &c.

The Lectures will commence at Three o'Clock.

Lecture I. Saturday, December 29. Substances generally—Solids, Fluids, Gases—Chemical affinity.

Lecture II. Tuesday, January 1, 1828. Atmospheric Air and its Gases.

Lecture III. Thursday, January 3. Water and its Elements.

Lecture IV. Saturday, January 5. Nitric Acid or Aquafortis—Ammonia or Volatile Alkali—Muriatic Acid or Spirit of Salt—Chlorine, &c.

Lecture V. Tuesday, January 8. Sulphur, Phosphorus, Carbon, and their Acids.

Lecture VI. Thursday, January 10. Metals and their Oxides—Earths, Fixed Alkalies and Salts, &c.

Non-Subscribers to the Institution are admitted to the above Course on payment of One Guinea each; Children, 10*s.* 6*d.*

The Weekly Evening Meetings of the Members of the Royal Institution will commence for the ensuing Season, on Friday the 25th of January, 1828, at half past Eight o'Clock, and will be continued on each succeeding Friday Evening, at the same hour, till further notice.

The Lectures will commence for the Season on Saturday the 2d of February, at Three o'Clock, by WM. THOS. BRANDE, Esq., F.R.S. Lond. and Edin., Prof. of Chemistry in the Royal Institution.

The Library of the Royal Institution is open for the use of the Members and Subscribers every day on which the House of the Institution is open; in Winter from Ten till Four, and from Seven till Ten in the Evening; and in Summer from Ten till Five, and from Seven till Ten in the Evening.

Mr. BRANDE and Mr. FARADAY will commence the Spring Course of their Chemical Lectures and Demonstrations, in the Laboratory of the Royal Institution, on Tuesday, the 12th of February, at Nine in the morning precisely. A Prospectus may be obtained at the Institution, or of the respective Lecturers.

In the Press, and nearly ready for publication,

A COLLECTION OF CHEMICAL TABLES, for the use of Practical Chemists and Students, in Illustration of the Theory of Definite Proportionals; in which are shewn the Equivalent Numbers of the Elementary Substances, with the Weights and Volumes in which they combine; together with the Composition of their most important Compounds, and the Authorities for their Analysis.

By WILLIAM THOMAS BRANDE.

TO OUR READERS AND CORRESPONDENTS.

We have to acknowledge the arrival of the following articles for insertion in this Journal, which will be duly attended to:—

On the Junction of Granite and Sandstone in Sutherland, by Dr. Mac Culloch.

On the Diluvium in Norfolk, by Mr. Rose.

On the Agency of Carbonic Acid, by Dr. Marshall Hall.

Continuation of the History of Horticulture.

On M[illegible]ecting Ships.

On the Aurora Borealis, by Mr. Kendall.

On the Ornaments of Architecture.

We have also been obliged to postpone the reviews and notices of several scientific works.

Our thanks are due to the suggestions of "A Constant Reader," who will perceive that we have attended to them.

The inferences of a correspondent at Manchester are wholly incorrect; the length of the communication, which would have occupied at least forty pages, and the refusal of the author either to abridge or divide it, were the reasons that induced us to return it. We are frequently obliged to refuse valuable papers upon similar grounds.

Dr. Mills's letter, from Bogota, has just reached us, and shall appear in the ensuing number of this Journal.

Mr. Johnson's paper on Saline Manures will appear in our next.

ERRATA IN LAST NUMBER.

In page	285,	line	33,	*for*	rest,	*read*	next.
	286,	„	22,	„	mean	„	mere.
	287,	„	15,	„	Russia	„	Prussia.
	do.,	„	do.	„	Siberia	„	Silesia.
	292,	„	12,	„	Property	„	Prosperity.

In the Ephemeris of Encke's Comet, published in the last number, the dates at the head of the columns are printed 1829 and 1830, instead of 1828 and 1829; the return of the Comet being expected in the course of the *present* year. The mistake was obligingly pointed out by Dr. Olbers.

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THE

QUARTERLY JOURNAL

OF

SCIENCE, LITERATURE, AND ART.

On the Beauties contained in the Oval, and in the Elliptic Curves, both simple and combined, generated from the same Figure or Disk. By R. R. Reinagle, Esq., R. A.

Being the subject of a Discourse delivered at the Royal Institution of Great Britain.

After an apposite discourse to introduce the subject, the first course taken, was to demonstrate the advantages of understanding the right use of geometrical terms in our descriptions of the varieties of shape, both in nature and art.

Every thing deserving the title of beautiful, and every grand object, assume an outline of definite character: these are to be found in the different classes of geometrical figures; the former in undulating lines of elliptic curves, and grandeur in angular dispositions of figure. All motion assumes a curved direction *. The primary and leading object of the discourse was to prove the fact of original beauty: and that a curved line was beautiful in an abstract point of view, free from all associations. For this purpose there were designed many diagrams on large black painted boards.

* A great number of geometrical diagrams were exhibited, from a single line, to angles, squares, oblongs, circles, ovals, cones, cylinders, spiral lines, and various serpentine lines, &c.

The explanation commenced with six or more parallel lines at equal distances, and equal length, in an horizontal position to the eye of the audience, *Fig.* 1; and another set of the same number of lines drawn perpendicular, *Fig.* 2: these were

Fig. 1.

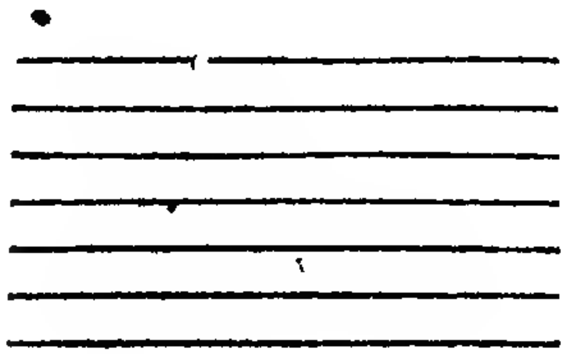
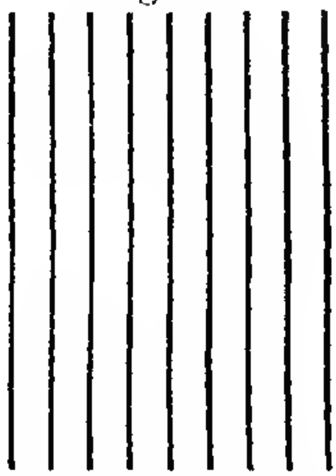


Fig. 2.



demonstrated to possess not the slightest character or principle of beauty in them, either as separate lines, or collectively, however many.

The next diagram consisted of six or more radiating lines from a centre, *Fig.* 3, and a corresponding number in an horizontal direction, but of unequal quantities; they diminished like a flight of steps, *Fig.* 4. It was then shown that the first

Fig. 3.

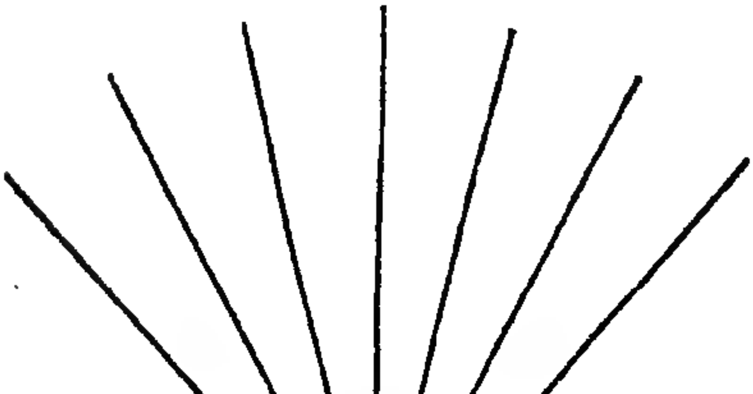
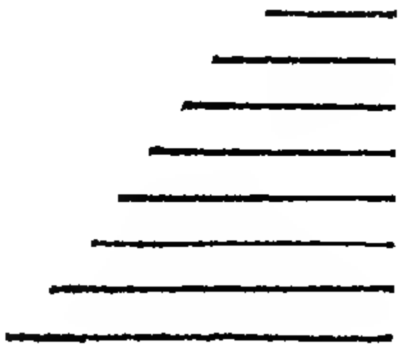


Fig. 4.

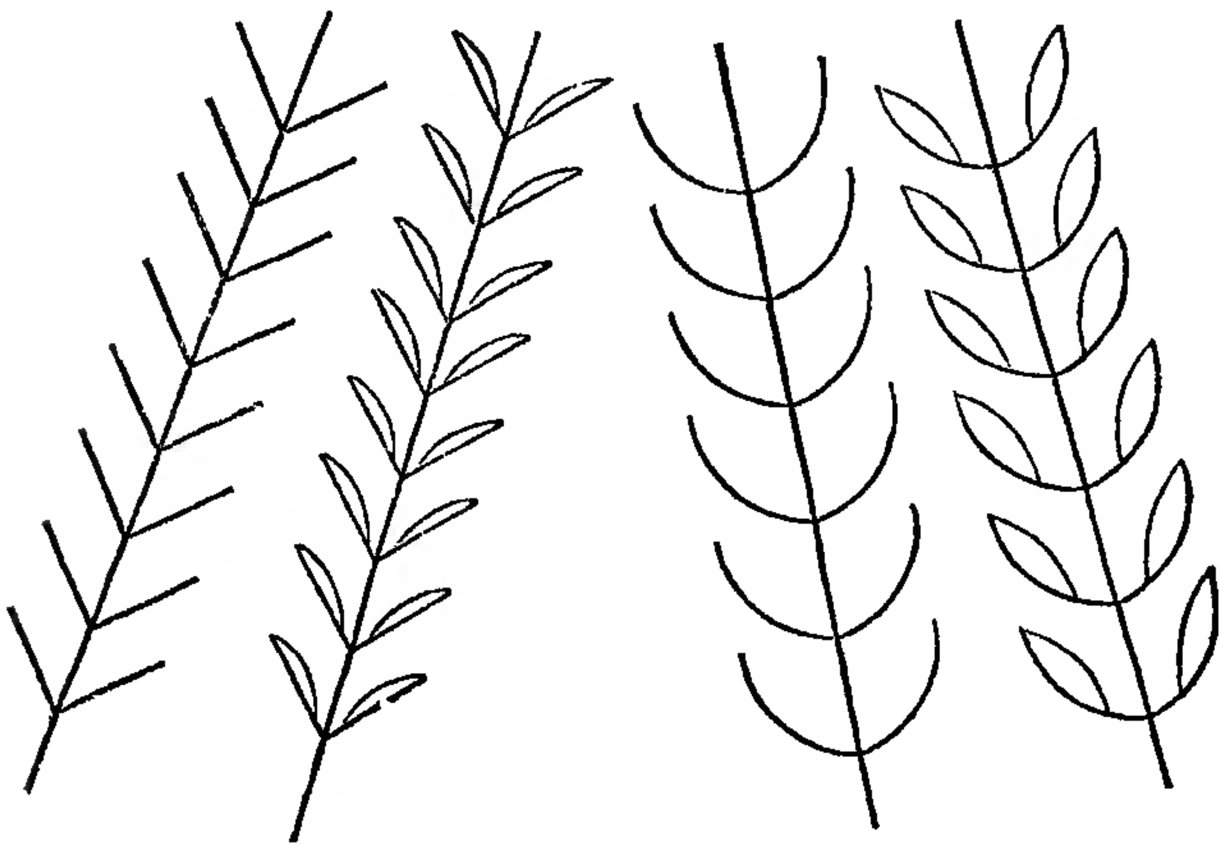


means of combining the six or more lines, which had been first drawn, so as to please the eye, without creating any geometrical figure, was the radiating principle. Our eye not only can tolerate that union of lines, but receive the impression as pleasing in character; while all lines parallel to each other, being right

lines, and viewed as a flight of steps, or pile of planks, opposite the observer, are disagreeable. Upon the former principle it is, that the rays of the sun, and rays of light generally, are so attractive and beautiful. It is from this circumstance that right lines drawn in an inclined position to the plane of the picture, derive an interest from the angles engendered through the imagination.

To follow up the principle by regular steps, and to open a clear view of the laws of beauty in lines, there were traced some inclined right lines (*Fig.* 5), with a regular set of right angles upon it, like the stems of leaves on each side. This exhibited no sort of beauty, nor any other advantage than mere combinations of formal angles. The next diagram (*Fig.* 6) was an inclined line as before, with similar angular projecting stems, to which were added elliptic curves on the upper side of each branch, that produced the form of a leaf. *Fig.* 7 was another inclined line, having oval curves upon it. Both these were shown to possess principles approaching to beauty, by progressive advances in combination and original structure. *Fig.* 8

Fig. 5. *Fig.* 6. *Fig.* 7. *Fig.* 8.



was an inclined line with the oval curves upon it; to which a similar addition of elliptic curves were adjoined to the stems,

as in *Fig.* 6. This addition made a new advance towards beauty. *Fig.* 9 commenced a more perfect principle of beauty, having an elliptic stem with oval branches rising from it, as in the others. If to this, the principle of gradation had been given, the eye would prefer it; I mean, by a scale of increase from the top to the bottom of the projecting stems: and if there had been superadded the external contour of a lengthened egg, like the form of a sage leaf, we should, step by step, advance into the region of beautiful character of exterior shape. *Fig.* 10 is a retrograde, showing how uncongenial angular forms are to curved lines, when producing ornament; at least how little our eye can bear the angular projections from the elliptic or oval turned stem. *Fig.* 11 was a curve of exactly the same disk, with the same oval stems, to which a small serpentine

Fig. 9.

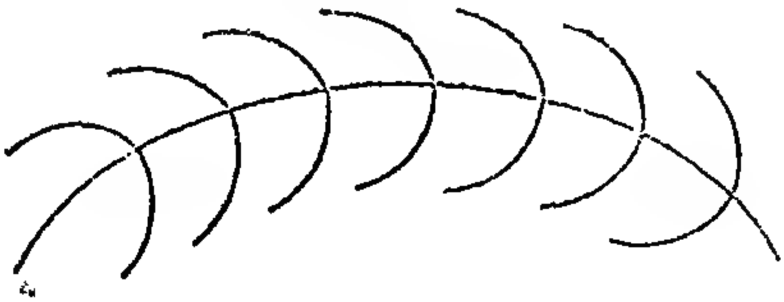
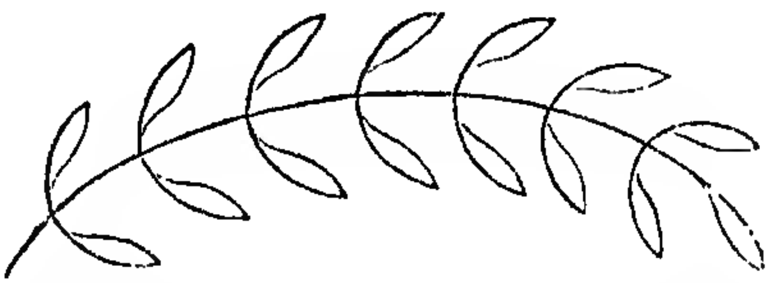


Fig. 10.

Fig. 11.

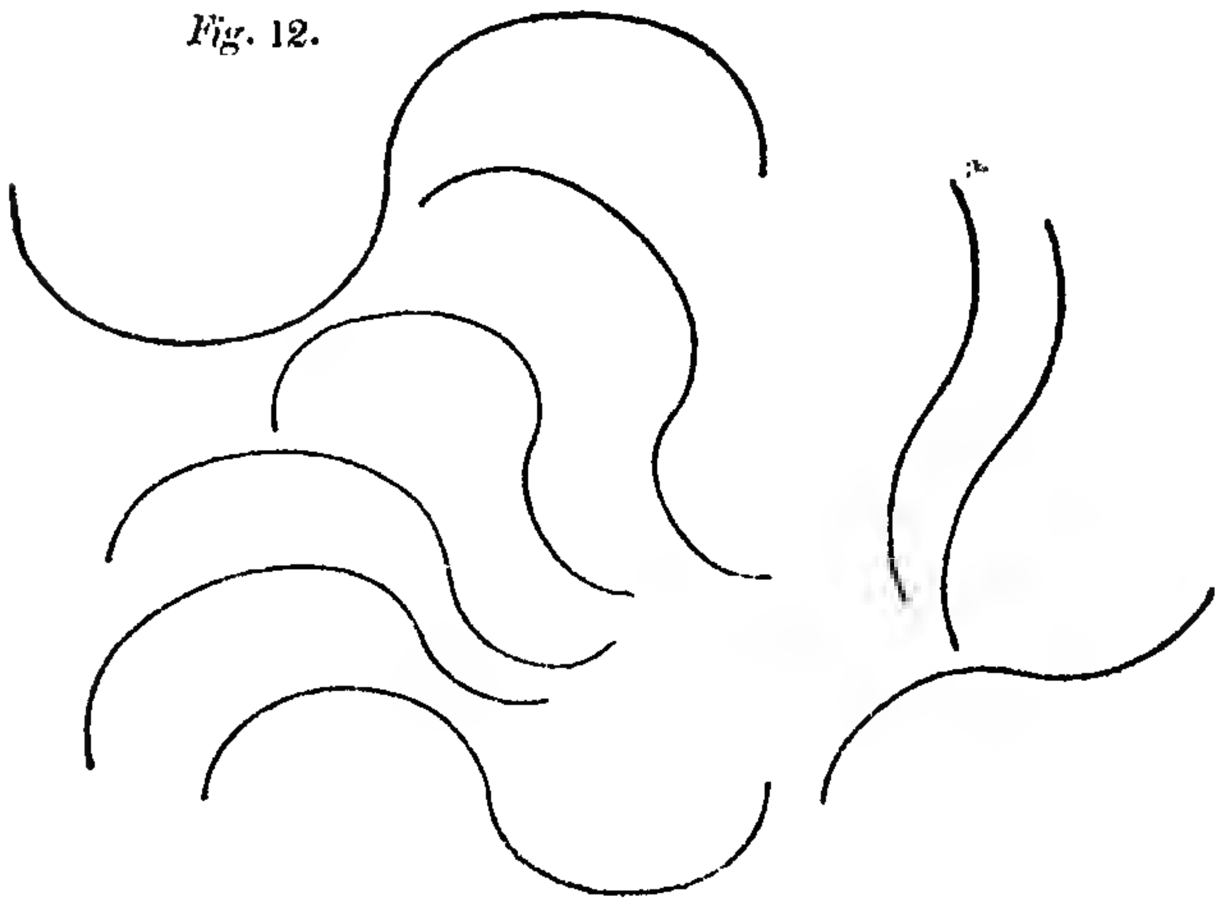


addition was made, expressing a leaf. Of all the last seven diagrams, this abounded with the greatest portion of beautiful lines, and is indisputably the most agreeable and beautiful. Combinations are like numericals; many of these forms, placed together with judgment and discretion, will attract us from the larger proportion of beauty that meets the eye at once, like a head of beautiful hair: one hair, however gracefully bent, cannot impress us like an entire lock of the hair; nor will this

curl charm us as the whole will on the human head. We owe to construction and combination all our pleasurable feelings of beauty: no person is allured by a single feature of any species of objects: but a thousand, or a million, arouses our anxious notice. Thus, the last diagram of the elliptic stem and the foliage upon it, exhibited, by the continuity of curved lines, the greatest approach to beauty, of all the figures presented to the notice of the audience.

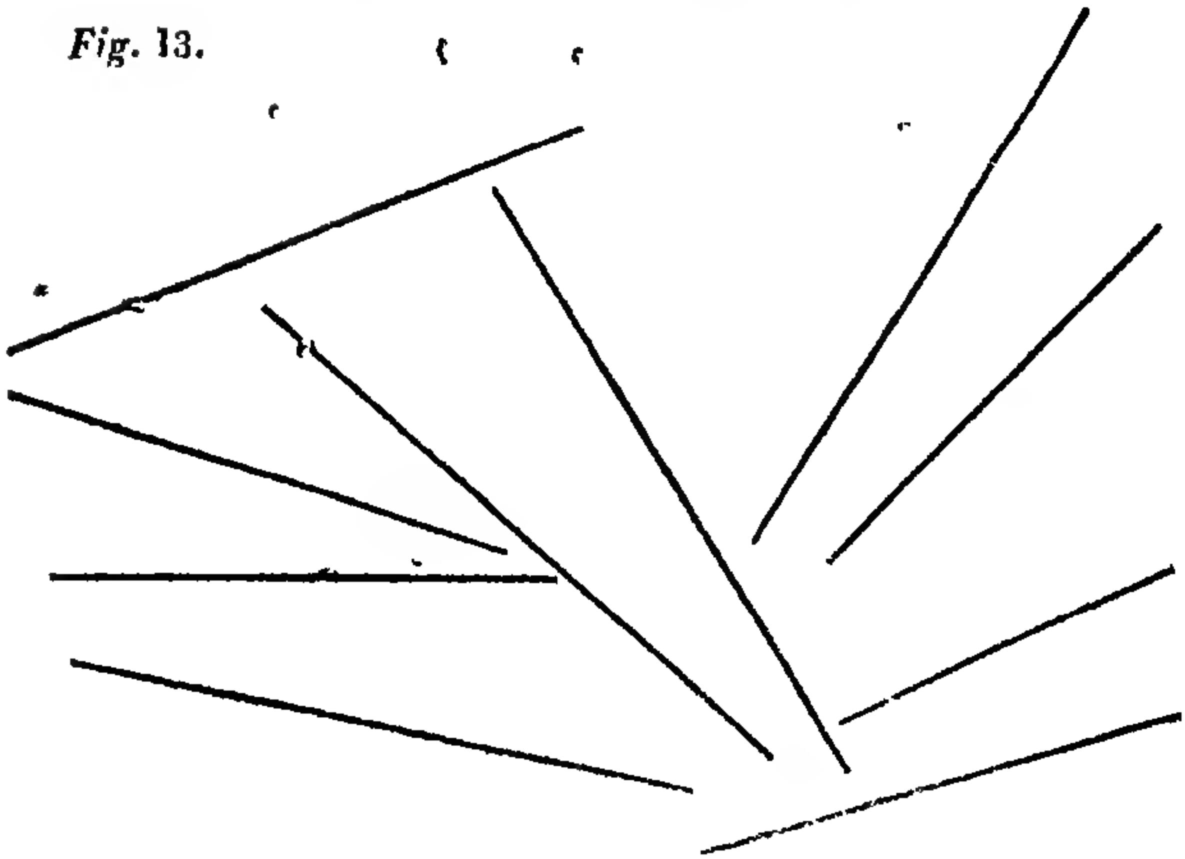
These preliminary designs opened the way for richer combinations; but the subject affording such an immense field of variety, I confined myself to the narrowest limits, and to one oval disk of seven inches transverse diameter, from which seven different designs were shown on paper. The first had a variety of serpentine lines placed at random, all produced by the disk of the oval just named, and the confluent lines of two such, placed side by side, or end to end, *Fig.* 12; which oval disk

Fig. 12.



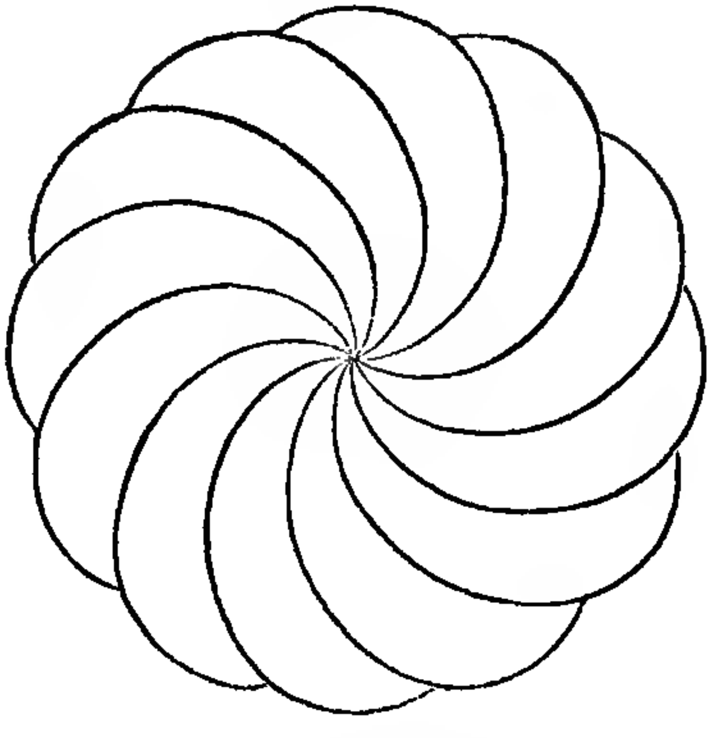
was put upon the lines to prove the construction. These lines, without expressing or forming any sort of figure, exhibit a set of elegant curves, of varied quantities of convex and concave, with which our eye will be more pleased than any set of right lines similarly distributed, as in *Fig.* 13, which follows.

Fig. 13.



Two other diagrams were placed before the company, each a circle of 12 ovals, from the same disk, revolved upon an axis, resting upon one end of the transverse diameter, (the lengthways of the oval,) which figure in the skeleton was a duodecagon. *Fig.* 14 is one of the diagrams, the ovals folding re-

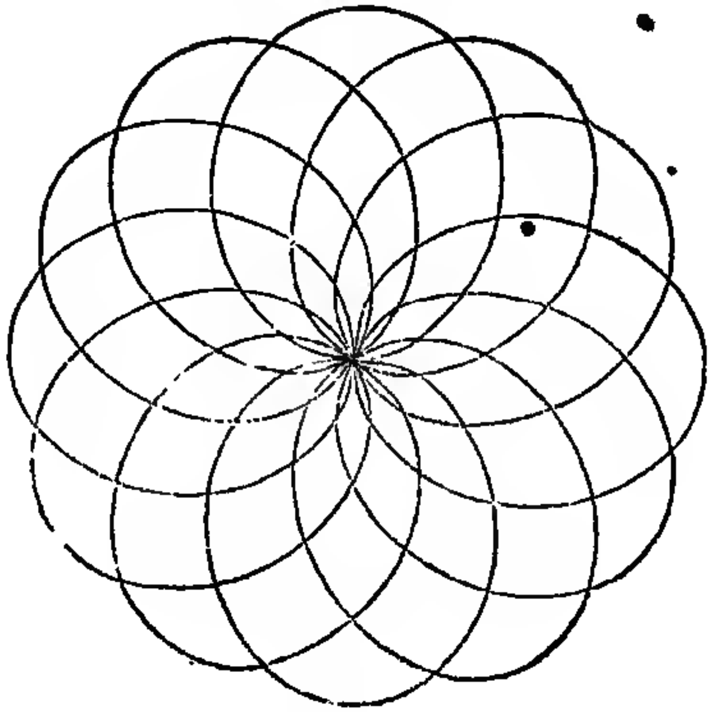
Fig. 14.



gularly over each other. By suppressing the continuity of the oval disk, where the lines would traverse, a very pleasing figure

is created. It may be easily converted into foliage, and can be amazingly varied in principle, by having fewer ovals, and making them revolve upon an arm or continuation of a line from the transverse diameter. *Fig.* 15 is the same diagram,

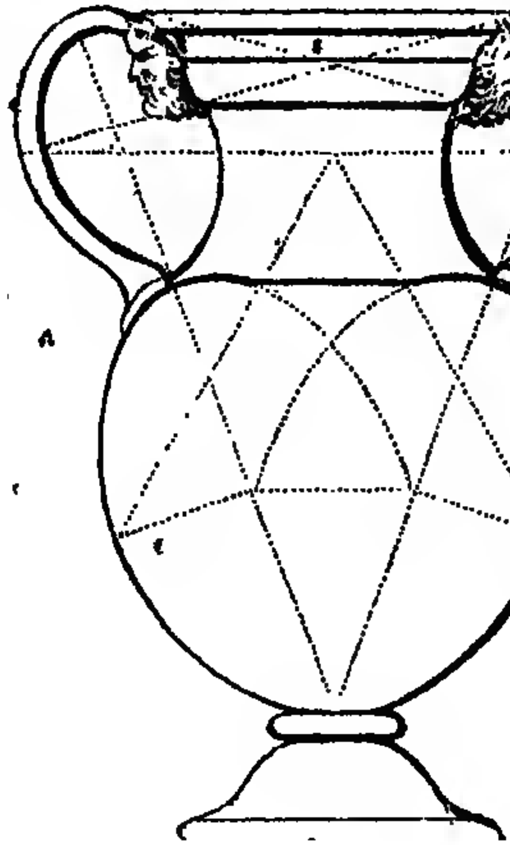
Fig. 15.



with all the oval lines described, which forms a figure of elegant intricacy; each member, or curvilinear subdivision, assumes a most agreeable shape: the whole, at the first sight, does not carry the evidence of being generated from the same disk. These agreeable figures may be varied to an extraordinary extent: the two that were presented were mere examples of some of the numerous changes that any given oval disk may create.

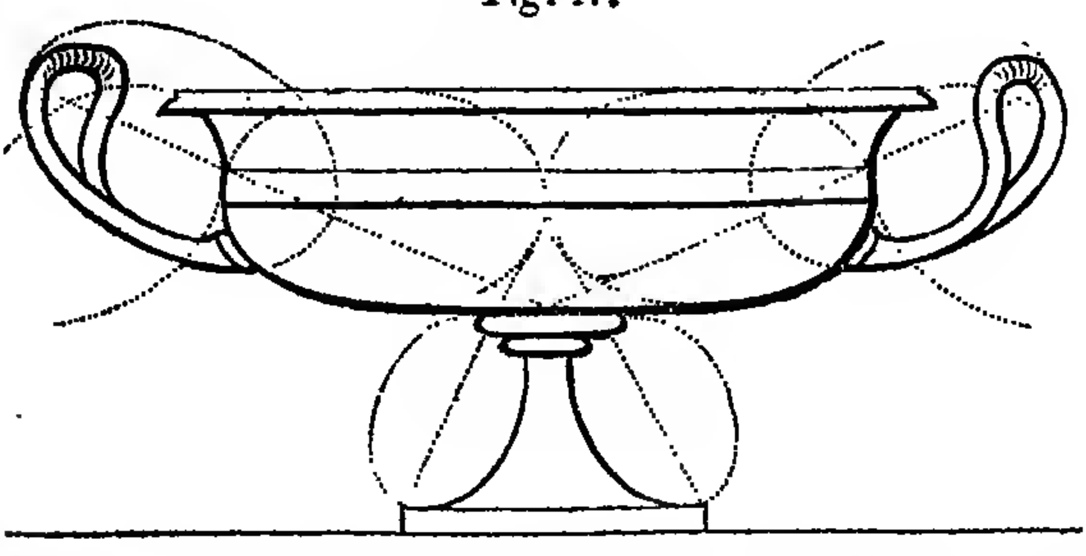
The objects next presented, were three vases of very dissimilar appearance, all produced from the same diagram of the oval; each in a separate drawing. The first was like a Greek vase with handles; its character established by employing certain proportions of quantities, in seven parts. The body has four parts, the foot or pedestal one; the neck two. The handles were regulated in the position and projection by lines drawn from the bottom of the vase, through the ovals which compose the outline of the two sides; and passing through the transverse diameter. These handles were made from an. oval that was the length of half the line of the transverse diameter, *Fig.* 16. The skeleton of angles that

Fig. 16.



govern the shape of this vase, is a very pretty figure of itself. The form does not proceed from any caprice of irregularity, but is consistent with rational organization, and symmetrical proportions. The figure of the plate sufficiently describes the mode of making the diagram without entering into the detail. *Fig.* 17 represents a tazza with handles: the same disk is

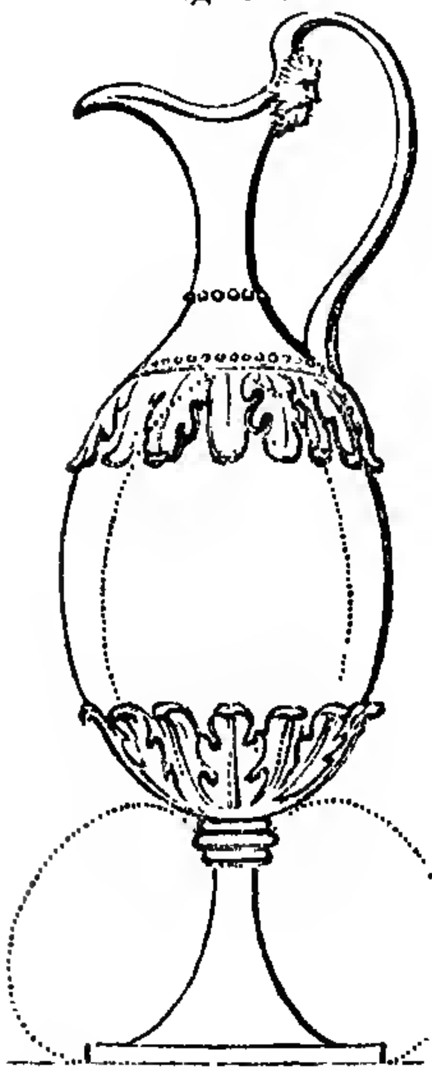
Fig. 17.



apparent, by the dotted lines that made the first vase. The ovals

are placed right and left of a central perpendicular line, dividing the cup in two parts; the transverse diameters meet in one line parallel to the base of the tazza; a dotted outline expresses the angular position of the handles: the concave lip of the tazza is made by the same oval disk, whose transverse diameter leads to the under line of the folding edge of the cup. The leg of the tazza is produced by the same small disk that served for the handles of the first vase. The body of the vase and the leg form two equal parts; the whole upper extent ought to be seven parts, so that it is seven and two*; the width of the base of the leg measures two parts, and the altitude three, of the seven parts. These proportions cannot produce any other than agreeable appearances, apply them as we may.

Fig. 18.



The third vase, exhibited an Hebe cup, with a handle, which presented a totally different appearance in form to the two previous ones. It was proportioned by similar principles: the larger disk made the body, inclined right and left upon the end of the oval. The neck and the leg were both made from the smaller oval disk; the dotted lines to the ovals of the leg sufficiently show the fact. The handle and concave lip of the cup were made by an application of the same disk. The altitude contained four parts. The body two parts, the leg one part, and the neck one other part; the handle rises one-eighth above: every portion of this figure is created by the two disks previously named. The foliage rises from below and descends from above, one-fourth of the whole height of the body

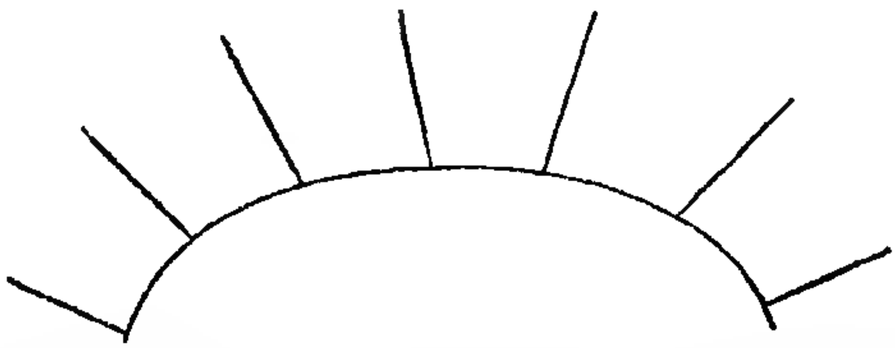
* The whole extent of the tazza, including the projection of the handles, should be seven parts; and the height of the vase two of such seven parts.

to the commencement of the concavity of the neck, where the beading runs round.

I remarked, that by adhering to regular proportional quantities of 1 and 2, 3 and 5, 2 and 5, 7 and 5, 7 and 2, &c., and using elliptic disks or curves, very great beauties are derived.

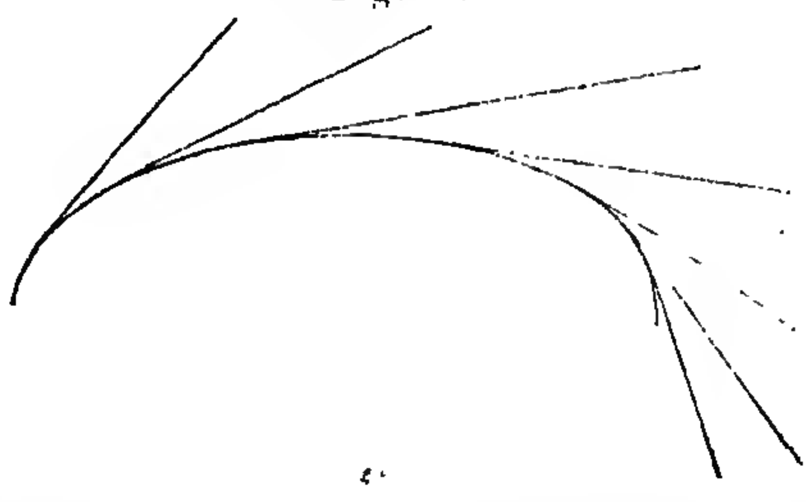
A skeleton of the tazza in angles was drawn on a black painted board, together with oval disks placed upon those lines, which clearly demonstrated the whole system of the construction. The explanation of these various diagrams necessarily involved a circumstantial description of each created figure, which were thoroughly analysed. Quantity and variety were particularly dwelt upon, as absolutely necessary to the production of perfect beauty; equalities being unfriendly to that symmetry which accords with nature. Some other diagrams were drawn, to show the inelegant appearance of radiating lines from the concave or convex half of an oval or an ellipse, *Fig.* 19:

Fig. 19.



but by drawing another convex half of an oval, and placing those lines as tangents, greater beauty was formed by the alternate changes and varieties of inclination of each tangent, *Fig.* 20.

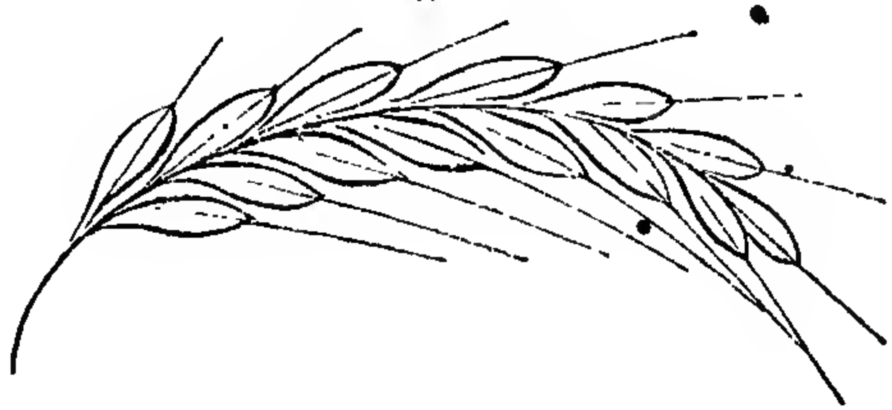
Fig. 20.



This was capable of an immediate adaptation to elegant vege-

tation; a few convex and concave elliptic curves added to each tangent, produced an ear of barley, or an ear of *rye*, the elegant construction of which, is rarely noticed in our remarks on nature, *Fig.* 21.

Fig. 21.



The discussion on these various designs being concluded, some important compositions of three great and renowned painters were produced, to corroborate what had been advanced in support of the native beauty of the oval and ellipse. Raphael's grand composition of the dispute on the Sacrament is in three grand oval curves.

The Doctors of the Church on the ground plan are ranged in an oval convex line; and the heavenly Choirs engage two concave oval shapes of the same proportion, but of unequal quantities. This is also a proof of a composition of parts, bearing two to one.

The facility of expressing such a composition, by being geometrical, is extremely easy.

The second illustration was the Aurora, by Guido, of the Aldobrandini palace. This was pointed out to depend upon an oval curve, and continued curvilinear details: the striking beauty of this fine composition is owing to its great and simple elliptic curve, which includes the whole group; the attendant hours have the principle of radiating to a centre of the oval: thus harmonizing and uniting forms congenial both to principle and nature.

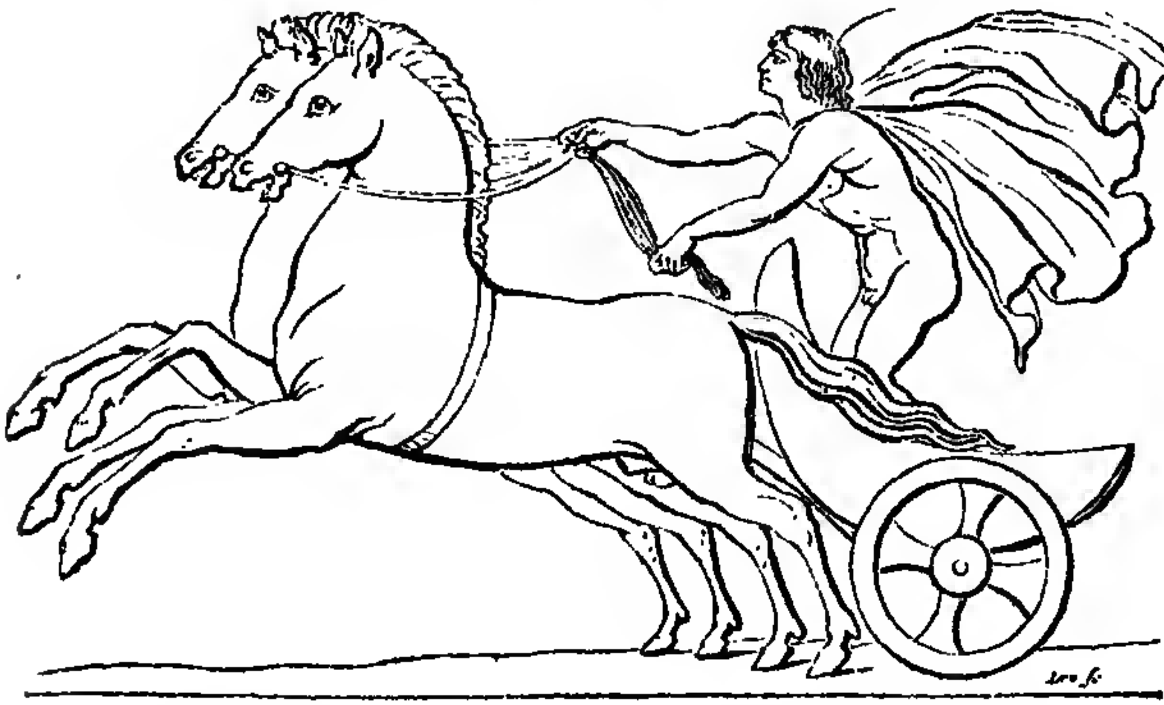
The third grand composition was by Rubens, the Coronation ceremony of Mary de Medicis, one of the grand Luxemburg pictures.

This very fine composition is contained in an oval con-

cave curve, and the figures in several points radiate to a centre. Some of the group pass the great leading line, but only to the degree and with the licence that a genius can effect, which destroys the too great, and the too palpable construction of the composition. The allegorical figures of Fame and Genius hovering over the royal personage, establish a centre to the oval, which prevents a void that would have been weak in the composition.

Three designs were next produced from Etruscan vases, to carry the evidence further, and to show the original source of the demonstrations of beauty in Grecian art. One was a charioteer driving a pair of magnificent horses of the highest spirit, *Fig*. 22. The composition is elliptic, and serpentine within.

Fig. 22.



The youthful conductor of the steeds is in a crescent or boat-shaped car, and his form is elegantly bent to meet the action and motion; his mantle flows behind in curved and serpentine folds, expressing the wind occasioned by the velocity of action. A more graceful or beautiful group and composition cannot be imagined.

The next design was a female in an elegant and very gentle serpentine action of the figure. Every portion of the outlines was elegant, from the varied succession of convexity and concavity; not a single angle could be traced throughout the whole

of this beautiful creature. She held in her left arm a very handsome oval vase; and in the other a sort of scarf with ribands, all serpentine in form. By her side is placed a young man selected from another Etruscan design.

Fig. 23.

The line of this figure was the outline of an ellipse; it is perfection in every respect; and the grace was shown to depend upon gentle curved lines of convex and concave, alternately blended, and confluent. The motion of ships at sea is described in gentle elliptic curves; the wings and plumage of birds assume the oval and elliptic curves; all the fibres of their feathers have that form; some flattened, others more rounded: the pine-apple and numberless fruits have all an oval character of outline.

Many take the character of eggs, pointed at one end, and large and blunt at the other extremity. The leaves of trees

have the oval shape more than any other; the bend of the branches, and the whole external form of many trees is oval.

There is no form of created things which may not be found to correspond in all its dependent shapes to ovals and ellipses of various disks, even objects which at first sight seem to contradict the possibility of meeting this system.

The lecture was closed by some extracts and quotations from Lomazzo, Dryden, Hogarth, Du Fresnoy, and the Abbé du Bos; the tendency of which was to show that lines had been mentioned, and had been written upon without any explanation given that could lead to certain conclusions. That all these authors attributed to supreme genius alone, and something of the divinely inspired character in artists, the power to produce those indescribable lines that affect the human eye so strongly. These lines I described as belonging to the oval and the ellipsis, and the confluent lines by conjunction and combination; that these indescribable lines, which from Plato to Dryden had never been detected or obtained a name; that puzzled all equally alike, are those alone I attempted, and I believe proved in this lecture, to be the elliptic combinations.

I stated that the great Greek artists confined themselves to certain rules and principles of unerring consequences in the production of beauty, grace, or grandeur in their figures; that all their compositions depended upon the same species of rule and order. I pointed out, that fashion is in all countries the destroyer of taste, that it unfits the mind for fixed principles; that where it dominates, *there* taste will be always fluttering and never settle, nor have a sure dominion. The Greeks, having no such vile tormentor to divert them from a pure course in their progress, arrived at the summit of perfection in every scientific pursuit, by following sure principles as their guides, and by never abandoning a path traced by nature, and matured by the most sublime philosophy.

On the Art of forming Diamonds into single Lenses for Microscopes.—By Mr. A. Pritchard.

[Communicated by Dr. Goring.]

Of the various improvements in Microscopes originated by Dr. Goring, that which he conceives to be the most important is the construction of single magnifiers from adamant. The details relative to this novel class of instruments, I have been induced to lay before the public. Single microscopes naturally aplanatic, or at least sufficiently so for practical purposes, possess an incontestable superiority over all others, and must be recognised by the scientific as verging towards the ultimatum of improvement in magnifying glasses. The advantages obtained by the most improved compound engiscopes over single microscopes resolve themselves into *the attainment of vision without aberration with considerable angles of aperture;* but against this must be set the never-to-be-forgotten fact, that they only show us a *picture of an object instead of nature itself;* now a Diamond Lens shows us our real object without any sensible aberration like that produced by glass lenses; and we are entitled, I think, to expect new discoveries in miscrosopic science, even at this late period, *from very deep single lenses of adamant**. I shall not fatigue my

* It seems generally admitted that, within a certain range of power not exceeding that of a lens of $\frac{1}{30}$th of an inch focus, the beauty and truth of the vision given by the new compound microscopes cannot be equalled by that of any single instrument, at least of glass. It is no less true, however, that the *picture* of the compounds, however perfect, is not like a real object, will not admit of amplification beyond a certain point with advantage. Under the action of very deep eye-glasses, the image of opaque objects especially, first loses its strong, well-determined outline—then grows soft and nebulous, and finally melts away in shadowy confusion. Let the experiment be made of raising the power of a compound up to that of a $\frac{1}{50}$th inch lens—then try it against the single microscope of that power (having, of course, the utmost opening the nature of the object viewed will permit). The observer, if open to conviction, will soon be taught the superior efficacy of the latter—for it will show the lines on the dust of Menelaus with such force and vivacity, that they will always be apparent *without any particular management of the light—nor can their image be extinguished by causing the illumination to be directed truly through the axis of the lens (as it always may in the compounds).* A due consideration of the teeth and inequalities on the surface of a human hair, together with the *transverse*

readers by describing the difficulties which were encountered in the prosecution of the design of making diamond lenses. Nature does not seem to permit us to produce any thing of surpassing excellence without proportional effort, and I shall simply say, that in its infancy the project of grinding and polishing the refractory substance of Adamant was far more hopeless than that of making achromatic glass lenses of 0.2 of an inch focus. I conceive it just to state that Messrs. Rundell and Bridge, of Ludgate-hill, had, at the time of the commencement of my labours, many Dutch diamond cutters at work, and that the foreman, Mr. Levi, with all his men, assured me, that it was impossible to work diamonds into spherical curves; the same opinion was also expressed by several others who were considered of standard authority in such matters.

Notwithstanding this discouragement, in the summer of the year 1824, I was instigated by Dr. Goring (at his expense) to undertake the task of working a diamond lens: (being then under the tuition of Mr. C. Varley, who was however at that time absent.) For this purpose, Dr. G. forwarded to me a brilliant diamond, which, contrary to the expectation of many, was at length ground into a spherical

connecting fibres between the lines on the scales of the curculio imperialis, viewed as opaque objects, will suffice to complete the illustration of the subject; though the last object is not to be well seen by that kind of light which is given by silver cups—and a single lens of $\frac{1}{60}$th inch focus can of course have no other. The effectiveness and penetrating faculties of simple magnifiers are invariably increased by an accession of power however great—that of compounds seems to be deteriorated beyond certain limits. An opinion may be hazarded that the achromatics and reflectors yet made *do not really surpass the efficacy of equivalent single lenses, even of glass, when their power exceeds that of a* $\frac{1}{20}$th lens, from $\frac{1}{20}$th to $\frac{1}{40}$th the vision may be about equal—but from $\frac{1}{40}$th upwards infinitely inferior.

The superior light of the single refraction can need no comment—and it is evident that there must be a degree of power at which that of the compounds will become too dim and feeble for vision,—while that of the single instrument will still retain a due intensity. For these reasons it is conceived that the close and penetrating scrutiny of lenses of diamond of perhaps only the $\frac{1}{200}$th inch focus, and an equal aperture (which their very low aberration would easily admit of,) must enable us to see further into the arcana of nature than we have yet been empowered to do. Glass globules of $\frac{1}{200}$th inch focus and indeed much deeper have been executed; but the testimony of *lenses* of diamond would certainly be far more respectable, and is at least worthy of trial and examination.—C. R. G.

figure, and examined by Mr. Levi, who expressed great astonishment at it, and added that he was not acquainted with any means by which that figure could have been effected: unfortunately this stone was irrecoverably lost. Mr. Varley having returned from the country, becoming now thoroughly heated with the project, permitted me to complete another diamond, which had been presented to me by Dr. G.: this is a plano-convex of about the $\frac{1}{20}$th of an inch focus: it was not thought advisable to polish it more than sufficed to enable us to see objects through it, because several flaws, before invisible, made their appearance in the process of polishing. In spite of all its imperfections, it plainly convinced us of the superiority which a *perfect diamond lens* would possess by its style of performance, both as a single magnifier and as the object lens of a compound microscope. After the completion of my articles with Mr. V., being entirely under my own command, I devoted some time to the formation of a perfect diamond lens, and have at length succeeded in completing a double convex of equal radii of about $\frac{1}{25}$th of an inch focus, bearing an aperture of $\frac{1}{30}$th of an inch with distinctness on opaque objects, and its entire diameter on transparent ones; it was finished at the conclusion of last year. The date of its final completion has by many been considered a remarkable epoch in the history of the microscope, being the first perfect one ever *made* or thought of in any part of the world*. I think it sufficient to say of this adamantine lens that it gives vision with a trifling chromatic aberration, but in other respects exceedingly like that of Dr. G.'s Amician reflector, but without its darkness: for it is quite evident that its light must be superior to that of any compound microscope whatever, acting with the same power and the same angle of aperture. The advantage of seeing an object *without aberration* by

* In Dr. Brewster's treatise on new Philosophical instruments, Book 5, chap. 2, Page 403—Account of a new compound Microscope for objects of Natural History—is the following passage: "We cannot therefore expect any essential improvement in the single microscope, unless from "the discovery of some transparent substance, which like the diamond "combines a high refractive with a low dispersive power." From which it seems certain that the Doctor never contemplated the possibility of working upon the substance of the diamond, though he must have been aware of its valuable properties.

the interposition of but a single magnifier, instead of looking at a picture of it (however perfect) with an eye-glass, must surely be duly appreciated by every person endowed with ordinary reason. It requires little knowledge of optics to be convinced that the simple unadulterated view of an object must enable us to look farther into its real texture, than we can see by any artificial arrangement whatever; it is like seeing an action performed instead of a scenic representation of it, or being informed of its occurrence by the most indisputable and accurate testimony.

Previous to grinding a diamond into a spherical figure, it is absolutely necessary that it should be ground flat, and parallel on both sides (if not a Laske or plate diamond), so that we may be enabled to see through it, and try it as opticians try a piece of flint glass: without this preparatory step, it will be extremely dangerous to commence the process of grinding, for many diamonds give a double, or even a species of triple refraction, forming two or three images of an object; this polarization of the light, arising from the primitive form of the crystal, of course totally unfits them for making lenses*. I need not observe, that it must be chosen of the finest water, and free from all visible flaws when examined by a deep magnifier. It was extremely fortunate for diamond lenses that the first made was free from the defect of double vision, otherwise diamonds *en masse* might at once have been abandoned as unfit for optical purposes. The cause why some stones give single vision, and others several peculiar refractions, may also arise from different degrees of density or hardness occurring in the same stone. Diamond-cutters are in the habit of designating stones male and female, sometimes a *he* and *she* (as they have it) are united in the same gem,—their *he* means merely a hard stone, and their *she* a soft one. When a diamond which will give several refractions is ground into a spherical figure and partially polished, it is seen by the microscope to exhibit a

* There are fourteen different crystalline forms of the diamond, and of this number, from the laws which govern the polarization of light, the octohedron and truncated cube are probably the only ones that will give single vision. It is unfortunately very difficult to procure rough diamonds in this country, so we are compelled to use stones already cut, and to subject them to trial in the way mentioned in the text.

peculiar appearance of an aggregation of minute shivery cristallized flaws, sometimes radiated and sometimes in one direction, which can never be polished out: I believe I could disstinguish with certainty a bad lens from a good one by this phenomenon without looking through it*. Precious stones, from their crystalized texture, are liable to the same defects for optical purposes as diamonds.

Having ascertained the goodness of a stone it must next be prepared for grinding; it will in many cases be advisable to make diamond lenses plano-convex, both because this figure gives a very low aberration, and because it saves the trouble of grinding one side of the stone. It must never be forgotten, that it may be possible to neutralize the naturally low spherical aberration of a diamond lens by giving it an improper figure, or by the injudicious position of its sides in relation to the radiant. When the lens is to be plano-convex, cause the flat side to be polished as truly plane as possible, without ribs or scratches; for this purpose the diamond should be so set as to possess the capability of being turned round, that the proper direction with respect to the laminæ may be obtained: when the flat side is completed, let the other side be worked against another diamond, so as to be brought into a spherical figure by the abrasion of its surface. When this is accomplished, a concave tool of cast iron must be formed of the required curve in a lathe, having a small mandril of about $\frac{2}{10}$ths of an inch in diameter, and a velocity of about 60 revolutions per second! The diamond must now be fixed by a strong hard cement (made of equal parts of the best shell lac and pumice-stone powder, carefully melted together without burning) to a short handle, and held by the fingers against the concave tool while revolving. This tool must be paved by diamond powder, hammered into it by an hardened steel convex punch: when the lens is uniformly ground all over, very fine sifted diamond-dust carefully washed in oil must be applied to another iron concave tool (I may here remark, that of all the metals which I have used for this purpose soft cast iron is decidedly to be preferred): this tool must

* As many amateurs of science might take an interest in the inspection of the peculiar effect these lenses have on transmitted *light*, I shall be happy to exhibit them, as also the perfect lens.

be supplied with the finest washed powder till the lens is completely polished. During the process of grinding, the stone should be examined by a magnifying lens, to ascertain whether the figure is truly spherical; for it sometimes will occur that the edges are ground quicker than the centre, and hence it will assume the form of a conoid, and thus be rendered unfit for microscopic purposes.

The spherical aberration of a diamond lens is extremely small, and when compared with that of a glass lens the difference is rendered strikingly apparent. This diminution of error in the diamond arises from the enormous refractive power possessed by this brilliant substance, and the consequent increase of amplification, with *very shallow curves.* The longitudinal aberration of a plano-convex diamond lens is only 0.955, while that of a glass one of the same figure is 1.166; both numbers being enumerated in terms of their thickness, and their convex surfaces exposed to parallel rays. But the indistinctness produced by lenses, arises chiefly from every mathematical point on the surface of an object being spread out into a small circle; these circles, intermixing with each other, occasion a confused view of the object. Now this error must necessarily be in the ratio of the areas of these small circles, which being respectively as the squares of their diameters, the lateral error produced by a diamond lens will be 0.912, while that of a glass lens of like curvature is 2.775; but the magnifying power of the diamond lens will be to that of the glass as 8 to 3, their curves being similar; (or, in other words, the superficial amplification of an object, with the perfect diamond lens before mentioned, is 22500 times, while a similar magnifier, made of glass, amplifies only 3136 times, reckoning 6 inches as the standard of distinct vision:) thus the diamond will enable us to gain more power than it is possible to procure by lenses of glass, for the focal distance of the smallest glass lens which can be well made is about the $\frac{1}{80}$th of an inch, while that of a diamond, worked in the same tools, would be only the $\frac{1}{200}$th of an inch.

If we wish to compare the aberrations of the two lenses when of equal power, the curvature of the glass must be increased; and as it is well known the lateral aberration increases inversely as the square of the radius, (the aperture and position remain-

ing the same,) the aberration of the diamond lens will only be about $\frac{1}{20}$th of that produced by the glass one, even when their thickness is the same; but as the curvature of the diamond is less, the thickness may be greatly diminished.

The chromatic dispersion of the adamant being nearly as low as that of water, its effects in small lenses can barely be appreciated by the eye, even in the examination of that valuable class of test objects, which require enormous angles of aperture to be rendered visible, which it is evident must be of easier attainment by diamond magnifiers than by any other sort of microscope.

A mathematical investigation of the spherical aberration of the diamond when formed into lenses, I hope to lay before the public at a future opportunity. The comparative numbers here taken from the longitudinal aberration are, I believe, sufficiently accurate for practical purposes.

18, *Picket-Street, Strand.*

Analysis of a newly-discovered Spring, at Stanley, near Wakefield.—By Mr. William West.

Mineral springs, dependent for their characteristic properties on carbonate of soda, appear to have been little noticed by chemists, and to have been still less attended to as curative means; at least in proportion to the multitude of cases in which that substance is administered in various other forms. Indeed the inference to be drawn from the silence respecting the modes of analysis adapted to such waters in our best elementary treatises, is that they have hitherto been very seldom met with. In one district, however, of Yorkshire, carbonate of soda is of frequent occurrence; it is found in the ordinary springs; often at the same time with substances with which, in artificial solutions, or when concentrated, it would be considered wholly incompatible; while at other times it is the predominant, or the only remarkable saline constituent. An analysis of a water of this kind, known by the name of the Holbeck Spa, has lately been published in the Annals of Philosophy, by my friend E. S. George; similar springs are found, I understand, as far

westward as Bradford; they are numerous from the borings in and near Holbeck; while eight miles south, a water similar in its character, but differing in containing about twice as much alkali in the same measure, has been discovered at Stanley.

About two miles from Wakefield, near the Aberford or York road, is an ancient mansion called Hatfield Hall; near the park or inclosure of which, in boring for coal, the spring in question suddenly gushed up, when the workmen had got to the depth of eighty yards, and has continued to run spontaneously, in all seasons, at the rate of six gallons per minute.

The water at the spring is limpid and very sparkling; the portion which is allowed to escape, deposits upon the trough and in the channel through which it runs a quantity of sulphur; the smell is that of sulphuretted hydrogen; the taste, from the stimulus of the bubbles of gas modifying the softness of the alkali, rather pleasant than otherwise.

The appearances presented by re-agents are,—

With tincture of soap, a slight opalescence.

Nitrate of silver, an abundant precipitate, partially re-dissolved by pure nitric acid.

Sulphate of silver, a precipitate only partially soluble in nitric or acetic acid.

Muriate of barytes, a slight precipitate.

Lime-water, a precipitate soluble with effervescence in acetic acid.

Oxalate of ammonia, no precipitate.

On boiling, a slight pellicle appeared, soluble in nitric acid.

Carbonate of ammonia, no precipitate, nor any on the subsequent addition of phosphate of soda.

The water restored the colour of litmus paper slightly reddened.

With tincture of galls and ferrocyanate of potash, no change.

With muriate of lime, the water remained unchanged until heated; but when boiled, a copious precipitate took place.

When concentrated by boiling, the water reddened turmeric paper, and effervesced strongly on the addition of an acid.

Nitromuriate of platina produced no precipitate, however concentrated the water might be.

The results of the previous experiments indicate the presence of

Soda,	Lime in small proportion,
Muriatic acid,	No magnesia,
Sulphuric acid,	No iron,
Carbonic acid,	No potash.

A. To ascertain the proportion of sulphuric acid, sixteen ounces by measure, previously saturated by acetic acid, were treated with muriate of barytes; the precipitate, washed and dried, weighed one grain; this indicates, in the imperial gallon, 3.2 grains of sulphuric acid, equivalent to 5.8 sulphate of soda, dry, or 13 grains crystallized.

B. For the muriatic acid; nitrate of silver, added to sixteen ounces of the water boiled, and the alkali previously saturated, gave a precipitate weighing 2.8 grains; reduced to the proportion in the imperial gallon, this amounts to 26.9 grains chloride of silver, equivalent to 11 grains chloride of sodium (muriate of soda.)

C. The crystalline pellicle separated from a pint of sixteen ounces, on boiling, weighed 0.2 grains.

This was carbonate of lime; but in the water the lime would be combined with muriatic acid, forming 0.22; or, in the imperial gallon, 2.1 dry chloride, or 3.75 crystallized muriate of lime.

D. The precipitate formed on boiling with muriate of lime, weighed from the pint, 3.6 grains; from the imperial gallon, 34.6 grains; showing the water to contain in that quantity a carbonated alkali equivalent to 53 grains of dry, or 59.5 crystallized bi-carbonate of soda.

E. Muriate of barytes, added to the water left on evaporating sixteen ounces to two, gave a precipitate weighing 8.2 grains; deducting one grain for sulphate of barytes, as found in experiment A, we have 7.2 carbonate of barytes; this indicates in the gallon 53 grains of dry, and 59.5 of crystallized carbonate of soda, as in the last experiment.

Lastly, a pint of sixteen ounces of the water, evaporated to dryness, furnished in three trials of saline residuum, weighed after short exposure to a dull red heat, six grains, or 57.6 from

the imperial gallon. Now we have seen that this would consist of

5.8	Dry sulphate of soda (exp. A).
11.	Chloride of sodium (— B).
1.9	Carbonate of lime (— C).
18.7	
38.9	
57.6	

The remainder, 38.9, having been converted by the heat into proto-carbonate of soda, is equivalent to 54.5 dry, 61 grains crystallized bi-carbonate, agreeing nearly with the quantities found from experiments D and E.

Following, as I do, that doctrine which supposes the bases to be distributed among the acids in a mineral water in the combinations which possess the greatest solubility, we must suppose the lime to be in the state of muriate; we shall then have to diminish the muriate, and increase the carbonate of soda: so that on this view, the saline constituents of an imperial gallon, in the state in which they exist in the water, are,—

Soda in combination with carbonic acid, equivalent to

Bi-carbonate or super-carbonate of soda	56	gr. dry.	62.5	crystallized
Sulphate of soda .	5.8	ditto	13	ditto
Muriate of soda (chloride of sodium) .	8.75	ditto	8.75	ditto
Muriate of lime . .	2.1	ditto	3.75	ditto

The gaseous contents of the water consist of variable proportions of carbonic acid, sulphuretted hydrogen, and carburetted hydrogen; the latter gas is continually emitted from the spring, in greater quantity than the water can absorb; and a portion of the other two also escapes from its surface. I have made many experiments on the gas, separated by boiling; but find the results, as I might anticipate, altogether inconclusive and uncertain. In waters containing, as at Harrogate, these gases with muriates or sulphates, boiling may be expected almost wholly to disengage them; but in this case the affinity of the soda in dilute solution, is likely to retain the carbonic

acid, and even to cause a decomposition of the sulphuretted hydrogen, so as to prevent our obtaining, in a gaseous form, the quantity really existing in the water, and imparting to it sensible or medicinal properties.

On the subject of medicinal qualities I am at all times cautious of giving an opinion: but I may observe, first, that as this spring is dissimilar to any of those which have already attained celebrity, so none of them can form a substitute for this; it is not Harrogate, or Cheltenham, or Buxton, or Tunbridge water: the alkaline springs of the West Riding, of which this is by far the strongest, stand as medicinal waters hitherto alone; the active ingredient, the bi-carbonate of soda, being spoken of in chemical works, as "rarely found in mineral waters."

Secondly, from the known properties of this substance, carbonate of soda, and the frequency of its administration in a long train of arthritic, calculous and dyspeptic complaints, the water must be highly useful as an anti-acid and as a diuretic; and as the advantages which native mineral waters possess over artificial solutions of the substances, in the great degree of dilution, and the impregnation with gases, and still more in the adjuncts of leisure, exercise, pure air, regulated diet and early rising, are of especial consequence in the latter very numerous class of diseases, those called stomach and nervous complaints; we may fairly suppose that such a spring will be found to be a valuable addition to those previously known, applying, as it does, to cases of such frequent occurrence.

Observations on the State of Naval Construction in this Country.

It appears that there is at present a tendency to improvement in every branch of science; monopoly in intellect may now be said to be vanishing; and empiricism is obliged to seek dark corners, to escape the light which is penetrating into regions from which it had but very lately been excluded. The administration, too, encourages advance of knowledge; yet notwithstanding these favourable circumstances, there still exists, in

some minds, an inaptitude of scientific perception, which induces unwillingness to acknowledge the advantage that results from the application of the exact sciences to the useful arts.

This neglect of scientific principles is nowhere more manifest than in the affairs of naval architecture, and it is not confined to the Royal Navy, but extends also to our mercantile shipping; and hence it is that our commercial marine is in some respects behind foreign nations, especially the Americans, in the formation of its ships; our merchantmen are, almost without exception, the most unsafe* and slowest ships in the world. The ship-owners, therefore, would do well to consider this circumstance, and endeavour to devise means of introducing science into the merchant yards. The establishment of the new university in the metropolis affords an opportunity of doing it at a comparatively small expense, by the foundation of Lectures on the theory of Naval Architecture; and the support even of a separate institution in the vicinity of the merchant yards of this great port, for the education of ship surveyors, would soon be repaid by the improved character of our merchant shipping.

If the science of Naval Architecture depend on certain physico-mathematical laws, as no doubt it does, it is monstrous to imagine for a moment that such laws can be developed by a flight of fancy, or that a man is born with an *intuitive optical* perception of the lines of least resistance, &c., or, in the jargon of the craniologists, that he has a naval-architectural bump on his skull; yet one would think that such was the case, when we see men, we cannot say philosophers, start up and loudly assert that they are in possession of the secret of construction; and they are believed because their hypotheses are never submitted to the examination of those who are capable of detecting their fallacy.

The Experimental Squadrons have, with a multitude of perplexing results, elicited, it must be confessed, at least an interesting fact, viz. that there has been an establishment seventeen years in this country, in Portsmouth dockyard, for the scientific education of naval architects, for the Royal

* By referring to Lloyd's List, it will appear, upon a moderate average, that *three* English merchant vessels are lost every *two* days!

Navy.* From the plan of education, as laid down by the Commissioners of Naval Revision in 1810, it appears that, to a requisite knowledge of the *practice* of their profession, the gentlemen composing this body of naval constructors unite a sound and competent one of its *theory*†.

It can only be from such a source that we can look for the improvement of our men of war, and it is to be regretted that every means should not be taken to avail ourselves of it: but unhappily such is the force of prejudice that, unless some alteration should be adopted in this institution, it will be in vain to expect advantage from it.

The objection urged against this establishment, namely, that the scientific education it gives to its members precludes them from the attainment of a due knowledge of the practical construction of our ships, is so absurd, that none but weak or jealous minds could ever have brought it forward. Shall it be laid down, in the present age, as an axiom, that a profound ignorance of the principles of his art is the one thing essential to the formation of what is generally meant by the term "practical man?" We contend that, having made, *in vain*,‡ a long and most indulgent trial of a system without science, if we may use such an expression, we must extend to one in alliance with it, a like patronage, before we can be allowed to pronounce a fair and legitimate judgment upon its efficiency.

But even in the peculiar path in which the naval architects educated at Portsmouth might be supposed to excel, we do not find that any opportunity is allowed them to come forward, nor shall we see this until some effort is made by the heads of our naval departments, to allow a broad and open competition to take place. It may be urged, that the learned Professor at Portsmouth (Dr. Inman) in himself includes all that can have

* See No. II. of the Naval and Military Magazine, published in June last.

† This will be readily acknowledged by those who will choose to read the "Papers on Naval Architecture," and the "Essays and Gleanings on Naval Architecture," two periodical works proceeding from the members of this institution.

‡ See the Third Report of the Commissioners of Naval Revision, and the Resolutions of the Society for the Improvement of Naval Architecture, in which the old system of providing ship-builders for the Royal Navy is condemned in the most unqualified terms.

possibly been taught or understood in the establishment over which he presides, and that therefore he is the representative of it in the late and present trials for the palm of excellence; but we cannot by any means assent to this: many of the students must have left his tuition seven, eight, and nine years, and must be between thirty and forty years of age; and it would be strange indeed, if during such a period, and in the prime of life and intellect, some of these, if not all, had not cultivated the science after their own bent of mind, and formed original ideas on the subject: we say, therefore, that Dr. Inman's constructions cannot be called the production of the establishment—they are merely the effort of one man, whose attention it appears is distracted by a multiplicity of occupations, and can only, along with the vessels of Capts. Symonds, Hayes, and Sir R. Seppings, be deemed criterions of the particular views of an individual.

Mysticism and ignorance always accompany each other; and we may reckon that in proportion as the latter disappears from amongst our ship-builders, so will the absurd vagaries of the former recede, and the subject be placed at last on the true principles of philosophical induction, instead of the caprices of imagination. We look forward, therefore, to this new body of naval architects for the expulsion of all quackery from their profession, and for the exposition not only of what we really do know, but also of what we do not know about it: this is the only way to arrive at truth, which should be the sole object of all investigation; but which we are afraid has hitherto been sadly garbled and perverted wherever it has had to do with naval architecture in this country.

But we repeat that we do not see that the nation is at all likely to benefit from the science or exertions of those gentlemen so long as they are placed in situations where a superior education can have no other effect than producing disgust and chagrin in the mind of the possessor; and if the institution at Portsmouth be designed for no better purpose than that of supplying house-carpenters, joiners, and still more inferior trades, with foremen, it had better be abolished. Some would regard it, as at present used, as a gross mockery on the public

at whose expense it is supported; it is certainly a cruel one of those who have been induced, by the fair and brilliant prospects held out to them of support and encouragement, to devote their lives to this branch of the public service.

But to return to the Experimental Squadron: it is with regret that we must conclude, upon a careful consideration, that, although the experiments are carried on with so much vigour and interest, they are evidently founded on imaginative views, and that there cannot exist any thing like legitimate data where so many failures and anomalous results obtain. Who can read the account of the first Experimental Squadron*, without immediately perceiving that the constructors of the contending vessels, however sanguine each might have been of the success of his particular fancy, met with nothing but the most perplexing results? We see sometimes one and sometimes the other vessel claim the palm of excellence, and finally leaving the subject as much in the dark as ever. This is the natural consequence of the non-application of inductive philosophy to the question before us, and the most important conclusion that can be gathered from the experiment is, that we have begun at the wrong end, and that it is high time to employ analysis instead of synthesis to effect the desired objects: for in the present state of the theory of naval construction in this country, there are yet no data existing to effect with precision and confidence the synthetical composition of a ship.

We cannot refrain here from noticing the paucity of information contained in the reports hitherto made on the first Experimental Squadron. The best one* is but little removed from a ship's log book, and in some respects is inferior to it: it is of such a scanty nature, that we can scarcely inform ourselves on any point, and that only in a *relative* degree, of the qualities of the vessels composing it: we cannot find out any mention of their *absolute* velocities on the different points of sailing, which is a most important omission. We are neither informed in what way the observations were conducted, whether they were made simultaneously or not: unless the former, any attempt at comparison must be very doubtful, if not entirely fallacious. Circumstances of wind and weather may very widely alter in the

* Vide No. 1 of the Papers on Naval Architecture.

course of a short time, and every endeavour at legitimate analogy be destroyed by such variation. We strongly suspect that this is one cause of perplexity; and another prolific one is the vague idea given of the strength of winds by nautical language. Nothing but the determinations of the anemometer should ever be allowed to appear in an account of such experiments. Every circumstance attendant on the quantity and trim of sail, the heeling, the rolling and pitching of the ship, position of the rudder, &c. should be accurately ascertained and *tabulated;* for it is next to an impossibility and a wilful waste of time to attempt to institute comparisons without pursuing a system of tabulated results, which should be kept in the same form on board each ship.

We must also express our regret that the scientific professor at Portsmouth does not appear to have ascertained the position of the centre of gravity of any of his ships, with regard to height, by the simple and easy experiment long known in principle, and described lately with geometrical rigidity in two or three publications by some of his pupils*. The knowledge of the position of this point would have placed him so far above his competitors, in so many important particulars, that we are surprised he should have thrown away his advantage, and descended to a level with his less scientific opponents. We are afraid that, here again, imaginative views have stepped in, and taken the sober mathematician from the only path by which excellence can be attained. We are at a loss to conceive how the stabilities of his ships can be said to be ascertained without the knowledge of the position of this point.

Some of the obscurity which pervades this difficult subject may be overcome, as to broad and general principles, by attentively and *coolly* observing the progress of marine architecture, since the introduction of cannon into naval warfare, and more particularly during the last century and a half. We shall then clearly perceive that the French, who, as early as the beginning of the reign of Louis XIV., employed men of first-rate talent in their naval arsenals, and neglected no opportunity for the

* Vide Annals of Philosophy, for November, 1826; No. 1 of the Papers on Naval Architecture, and No. 11 of the Essays and Gleanings on Naval Architecture.

advancement of science in them, increased and kept increasing the dimensions of their ships, more especially the length, the ratio of which to the breadth has been augmented by them from about $3\frac{1}{4}.1$, to 4.1 within the last century. While this principle was acted on, the improvement of their ships was gradual; and by referring to our own progress in the art, in tardy imitation of the practice of the French, we shall likewise conclude that our navy has derived precisely similar advantages from the same causes. Here we have at once two grand but *concurring* results derived from an experiment, not made on one or half a dozen different vessels, but on the whole navies of the two most powerful maritime states in the world: and if to these we choose to add the result of the practice of the same means on the Spanish and other navies, we might surely be warranted in saying, from this broad but *certain analysis* of *facts*, that, in relation to the hull, the *general increase of dimensions, with a greater relative length*, is one cause of the improvements that have been made in the sea-going qualities of the ships composing the fleets of the present maritime powers: the question therefore that remains to be decided on in relation to this principle is, whether we have arrived at its utmost practicable limits, or rather, whether we have arrived at the *maximum* of improvement it is capable of producing.

This brings us again to the experimental squadrons, as far as they are connected with, and illustrative of, our observations; and the first question naturally put forward about them is, whether there be any thing very peculiar in the formation or dimensions of the rival vessels? We suspect that the answer cannot otherwise than disclose, that neither in principle, dimensions, nor in the formation, can they be said to differ very materially from each other, or from ships of the common construction: indeed we perceive in some a retrogression of ideas and a violation of the principle, that the increase of the ratio of the length to the breadth, in conjunction with a general increase of dimensions, has been a predominant cause of improvement. The fact also of so immaterial a difference necessarily includes a system of masting and sails equally confined, and totally inadequate to produce any great superiority of sailing over ships to which they are so nearly equal in principal dimensions.

After so many years of trial with the present nearly invariable set of principal dimensions, during which period it may be said, that every possible contour of hull has been experimented on with them, we are inclined to think that almost all has been done that could be done under such restrictions, and that some great step must be made in one or other of the principal dimensions themselves, with correspondent alterations in the masting, before we can expect to see a decided and great improvement in the sailing of our ships. The depth is an element which has arrived at its limit from very apparent external causes; but the length and breadth remain to the skilful constructor without any such clogs to his endeavours; and he has only to accommodate their relation to each other in the manner most conducive to velocity, which in our opinion is the very capital object of naval construction, both in ships of war and of commerce. That it is so in the former, no one will, we apprehend, on due reflection deny; but there will be many who will assert that it cannot be obtained, in the latter, without a sacrifice of capacity, which will defeat the object of carrying large cargoes: to this we may reply, that if a vessel with an expense of one quarter the capacity can make *three* voyages instead of *two*, will not the merchant be still a considerable gainer in capacity, and still more so by a ready return of his capital*?

All observations on well-conducted experiments concur in proving that velocity is gained by increasing the length, to a much greater degree in relation to the breadth, than has ever yet been done in ships; and that the increase of the same element contributes to their weathering powers is too obvious to need insisting upon: it is also generally advantageous, when not carried to an extent which would seriously retard the manœuvring of the ship. This limit has not yet by any means been determined; for it must be recollected, that although the additional length increases the resistance to rotation about a vertical axis, yet the power of the sails to give rotation about the same is also increased, although not in so high a ratio. The power of the rudder to produce rotation is also greater in a long ship than in

* Foreign nations, and more particularly the Americans, find their advantage in having swift merchant ships, and therefore our assertion is warranted by facts.

a short one, not only on account of the greater distance it is from the axis of rotation, but also on account of the greater velocity, and the more direct impulse of the water on it.

The increase of the ratio of the length to the breadth to produce velocity should not interfere with the increase of breadth necessary to produce stability or capacity; for both these qualities, varying as higher powers of the breadth, a very small increase of breadth may be attended with a considerable increase of length. If we compare the Caledonia's (120 guns) dimensions with those of the Royal George and Queen Charlotte*, of 1788 and 1789, we shall find, that 13 or 14 times as much length as breadth has been added to the first rates of our navy. If we refer to the dimensions of the Commerce de Marseilles, and those of the next preceding three-decker of the French navy (for instance, the Ville de Paris†, taken in Lord Rodney's action), we shall find that the French naval architects gave in her 21 times as much increase to the length as to the breadth. If this could be done with safety in a three-decked ship, with such a vast top weight, much more could it be carried advantageously into effect in ships of two decks, and frigates; but we do not find, in the latter classes of the ships of the French navy, the increase of length to go beyond six times that of the breadth. If we refer to the Old Bellerophon, built in 1772, and the New Bellerophon, built in 1819, we shall find an increase of 24 feet in length, to 1.58 feet increase of breadth; or the former more than 15 times the latter‡.

To those who oppose the objection that a greater length than at present used would make the manœuvring of a ship too slow, we answer, that as the Caledonia and the present first rates of our navy, although from 10 to 15 feet longer than our two-deckers, are found to be capital ships in this respect, there is a sure ground to believe, that the addition of 20 feet in length to the present two-deckers would not render their cele-

* Caledonia, length 205 feet, breadth 53.5; Royal George, length 187 feet, breadth 52.33 feet; Queen Charlotte, length 190 feet, breadth 52.33 feet.

† Ville de Paris, length 185.62 feet; breadth 52.7 feet; Commerce de Marseilles, length 208.33 feet, breadth 54.79 feet.

‡ Old Bellerophon, length 168 feet, breadth 47.33 feet; New Bellerophon, length 192 feet, breadth 49 feet.

rity of evolution less than that of the three-decker; and since, from the reduction of weight aloft, the centre of gravity would be lowered, and the displacement required to be less, a somewhat smaller breadth might be allowed to a two-decked ship of 206 feet long, than to one of 196 feet (especially since the quantity of sail, remaining the same, is lowered by one whole depth between deck), a smaller midship section would be, *cæteris paribus*, required; the velocity of this ship might be considerably increased. Nothing however can be precisely determined on, with such a complication of circumstances, beyond a general idea. Calculation and a strict analysis of ships must be resorted to, in order to fill up the outline of our reasoning.

But for the same reason that we imagine that an addition of 20 or perhaps 40 feet would not sensibly injure the celerity of manœuvring of our two-deckers, we should think that the same increase of this dimension might be tried without much risk to our first rates, with an increase of breadth not exceeding $\frac{1}{20}$th part that is given to the length.

We repeat that the very capital object of the science of Naval Construction is *velocity*, and we are decidedly of opinion that it is attainable in a much higher degree than at present, without compromising other necessary qualities, for which we have the concurrence of facts as far as they go.

The Anglo-Americans, in the last war, took every possible advantage suggested by views similar to those we have been adverting to, in the construction of their large frigates. They had, it may be said, to create a martial navy, and they had to oppose it against fearful odds; but, free from the prejudices and errors so blindly cherished by their opponents, and which constantly oppose reform by always declaring the present practice to be the best, they did not retread the old path, but began at its last step, and boldly advanced on this principle into all the branches of the art. They built vessels upon the most enlarged dimensions, and of a superior weight of metal, and gave an increased ratio of length to the breadth. The result of such a procedure, justified the confidence of the American naval architects in only *one* maxim, founded upon the *scientific* observation of facts, and may give us a faint idea of what might be effected by a still more enlarged and mathematical analysis.

Our frigates were so inferior to theirs in every way, that they brought nothing but disasters upon us, excepting in the action between the Shannon and Chesapeake, and one or two others, where, assured by their previous successes, our gallant opponents threw away the advantages possessed by their ships, by coming to close quarters at once, and deciding the contest hand to hand.—Our ships of the line could never bring these frigates to action, and owing alone to their extraordinary sailing, did they evade and mock a large British fleet. We were finally obliged to build 60-gun frigates after their method, but when it was *too late* for the exigency of the period; and thus it has ever been our fate, for want of science in the constructors of our navy, to follow the steps of our enemies at a humble distance, and to be only then driven out of the old track by a terrible experience of its inefficiency.

Nor have the Americans stopped here;—Mr. Huskisson plainly tells us that "America is, year after year, augmenting its military marine, by building ships of war of the largest class*." According to Capt. Brenton, they have built a first-rate † of 245 feet length on the gun deck, and 56 feet broad‡, to carry 42-pounders on the lower deck, and 32-pounders on the other decks.

Our small class of 74-gun ships lately converted into frigates carrying *fifty* 32-pounder guns, we are fearful can only produce disappointment if ever brought against the American frigates (not by conversion, but by *construction*), which carry ***sixty-two*** guns of the same calibre, and are 180 feet long on the gun deck.

We must not forget also that our active neighbours the French have now adopted a most formidable description of

* Vide this gentleman's speech on the Shipping Interests in the House of Commons, May 1827.

† Called by Capt. Brenton the Ohio; but it appears from Lieut. De Roos' personal narrative, just published, that the Ohio is a two-decker of 102 guns. It is to be supposed, therefore, that the three-decker of 135 guns, called the Pennsylvania by the latter, is the ship alluded to by the former. It is a matter of great regret that Lieut. de Roos has not presented us with the precise dimensions of these ships.

‡ These dimensions carry the ratio of the length to breadth above 4½ to 1.

frigates, with curvilinear sterns*, and many other important improvements. They mount 60 guns and carronades—viz. 24-pounders on the gun deck, and 36-pounder carronades on the flush deck.—The former calibre is equivalent very nearly to 26, and the latter to 39lbs. avoirdupois.

When we reflect on these circumstances, we cannot but feel surprised that so many frigates of inferior force and dimensions should be building in our dockyards. In time of emergency they will only bring on us a repetition of former disasters and deficiency. We contend that, instead of building ships of only *equal* force to those of our rivals, and thus *waiting* for the developement of *their* designs before we can venture on a single step, we should build beyond them in every respect. It must and ought to be recollected, that peace in these matters produces a contest of intellect, and those will have the advantage in it who attack instead of standing on the defensive. We ought to lead the way, and to be at the head of the maritime world, not in *number* alone, but also in the *individual force and qualities* of our ships.

Having expatiated on the advantages of an increased ratio of length to breadth in relation to the hull of a ship, we will just glance at some of the principal effects it would have upon the masting and sails; and here again we conceive that Professor Inman has, in common with many others, relinquished the many good effects resulting from it, for the inadequate one, of being able to carry a somewhat greater quantity of sail, which must necessarily be lofty, and which, (setting aside this detracting circumstance,) as the velocity of a ship varies only as a *fractional* power of the surface of canvas spread, cannot produce the degree of fast sailing to be wished for, but at an immense and impracticable quantity of sail†.

A greater proof of the inadequacy of the present system of

* The French Admiral Willaumez, in his "Dictionnaire de Marine," published in 1820, says under the article *Frigate*, that as far back as 1804, he had proposed a plan for a frigate of the largest size, with a round stern, wherein the quarter galleries were suppressed: the first frigate upon his plan was built at Brest about 1821.

† As the square root, so that to get *twice* the velocity, *four* times as much canvas must be spread; and this is the most favourable estimate that can be made.

lofty sail cannot be cited than the fact of its not procuring, under the most favourable circumstances, a rate of sailing rarely exceeding one-fourth the velocity of the wind.

As the number of masts should be so regulated as to create facility in managing the canvas, which is well known to be at present hardly manageable in a gale of wind, on board large ships, from the enormous size of each individual course and topsail, we should not hesitate, therefore, to have *four* vertical masts, as recommended by Bouguer, instead of three, in ships built in accordance with the principles we have been discussing. This would, *cæteris paribus*, require shorter masting and smaller yards, and the sails being much less, individually, would be more easily managed and not so liable to accidents.

From what has been said, and the actual experiments now pending, it is apparent that the theoretic construction of ships is at a very low ebb in this country; yet a fine opportunity now presents itself, if we choose to avail ourselves of it, for rescuing the nation from this generally acknowledged odium. Let a proper use be made of the corps of Naval Architects we have, somehow or other, at last got, and let their exertions, under a degree of encouragement equal to that bestowed on the old ship-builders *in vain* for so long a period, be directed towards the improvement of their art. If they fail, they cannot claim the excuse of having their endeavours repressed; if they succeed, as no doubt they will, in advancing their profession to something beyond mere carpentry, we shall be enabled to bid adieu to the old and *ruinous* method of blundering, under the reign of which nothing but disappointment can ever be reasonably expected.

We have seen and do still see the immense advantages derived by our country from the encouragement of those branches of science connected with its manufactures and agriculture; and if we wish to keep our present superiority, we must follow up vigorously this principle in all its universality. To the cavils of ignorance and bigotry against such a mode of proceeding we would answer, in the words of one of the most enlightened members of the present administration, "This country cannot stand still, whilst others are advancing in science, in in-

dustry, in every thing which contributes to increase the power of empires, and to multiply the means of comfort and enjoyment to civilized man."*

It is to be hoped, therefore, that His Royal Highness the Lord High Admiral will extend to this most important national institution, the School of Naval Architecture, the same vigilant and scrutinizing eye that every other branch of our naval system is at this moment experiencing from him, and that he will extend to it that fair play and encouragement which has hitherto been denied to it. As a seaman, he can fully appreciate and understand how much the bad qualities of a ship may neutralize the best exertions of the most experienced and skilful sailor; and, on the contrary, what a degree of confidence may be insured in naval operations with excellent ships. We feel persuaded, therefore, that he will not allow others to think for him in a matter of so much national importance, and thus allow private ends to interpose to the disadvantage of public views; but that he will investigate and judge for himself. We would humbly suggest to His Royal Highness to inquire into the individual acquirements and productions, both of a *theoretical* and *practical* nature, of those who have been educated in this establishment, and he would soon be able to decide whether they be fitting or not for the important task of constructing our ships, and for the confidence and protection which we think we have shown has hitherto been ill-advisedly withheld from them. Such a line of conduct would very soon carry our naval architecture to a pitch of excellence *worthy* of imitation, and instead of being indebted to foreigners for models, we should be able, with just pride, to point to the productions of British science and intellect in this noble art.

* Vide Mr. Huskisson's speech on the Shipping Interests.

On Malaria. No. II.

[Communicated by J. Mac Culloch, M.D., F.R.S., &c. &c.]

Having pointed out, in the former paper on this subject, the nature of the soils or places, of whatever description, by which malaria is generated, it remains to notice a few other circumstances connected with its natural history, a knowledge of which is essential for the purposes of prevention; and finally to describe such modes of prevention, applicable to these several circumstances, as have been found useful in guarding against the attack of diseases from this cause. Under the first head, there remain to be considered, the effects of climate and season; the changes which occur in the production and propagation of malaria, from various natural and artificial causes; and also, the various modes in which it is propagated.

It has already been remarked, that a certain elevation of temperature was necessary to the production of this poison, though what the precise degree is, has not been ascertained; and as this is, chiefly, what distinguishes the regions or periods of the year which generate malaria, I need not make two divisions of season and climate. If, however, this temperature is not fixed, it will perhaps suffice for our present purposes to say that the greater part of Scotland, whether as to climate or season, seems incapable of generating the disease from this cause; though there are exceptions of a permanent nature, or exceptions of climate, as was perennially true of the Carse of Gowrie before its drainage; while there are others which happen when, as in the last year, there has been a peculiarly hot summer, and which are exceptions of season.

And thus it is as to more northern regions; where a hot summer becomes more than an equivalent for an average low temperature; as an example of which, there is no place where intermittents are more severe and abundant than at Stockholm. But the extreme of evil from this cause occurs, as is well known, in the tropical climates; appearing almost proportioned to the heat of the climate, and what is important to observe to the moisture also. The destructive effects of certain parts of Africa, India, America, and so forth, are familiarly known; and

it is in these countries especially, that the diseases from this source constitute nearly the entire mortality of the human race. And thus, for Europe, it is in Spain, Italy, and Greece, and chiefly on their Mediterranean shores, that the activity of malaria scarcely yields to that of the intertropical climates; while in France, Holland, Germany, Hungary, and with us, in a far less degree, the production will be found regulated by the heat of the summers, all other circumstances being the same.

And if we thus account for the variations in the quantity and virulence of diseases in any given country, for noted seasons of epidemic in the countries which I have just named, and for the great prevalence of fevers among ourselves during the last few years, and particularly in the last summer, there is another point of scarcely inferior importance to be taken into the consideration, independently of that which relates to peculiar winds as connected with the propagation of this poison;—and this is, moisture.

I need not repeat that water in some form is necessary to the production of that peculiar vegetable decomposition which is the source of this poison; and so true is this, that even in the tropical regions, the diseases from this cause are nearly unknown in districts of peculiar dryness, as they are in the drier seasons of those conutries. Thus, for example, Egypt is free from such fevers, except at the period of the subsidence of the Nile, unless where, as at Damietta, the cultivation of rice is pursued; and the same is true of Mesopotamia very remarkably: and if I dare not extend these illustrations, I must remark that in all these cases, the action of moisture is twofold, inasmuch as it not only accelerates vegetable decomposition, but renders the atmosphere a fitter conductor of this poison.

Taking these two causes of the increase in the quantity and in the action of malaria, we can explain many particulars which relate to its power in producing diseases: and as the knowledge of these is important as far as relates to the main object of this paper, prevention, it becomes necessary to explain them at a little more length.

As to season, the simplest case is that of the intertropical climates: and Africa offers the plainest instance among the

whole. There, the malaria and the fever commence at the moment the rain falls; diminishing as the ground becomes thoroughly wetted, and recommencing as it dries. The explanation of all this ought to be obvious; and the same analogy governs all the hotter climates, as, though less conspicuously, it does our own Hence we explain, both as to our spring and our autumn, the effects of heat following rain, or the reverse, and the diseases which are consequent on those changes: and thus it is, though more remarkably, in Italy, that a rainy autumn increases the number and severity of fevers; or, if the summer has been unusually dry, that they often do not appear till the commencement of the autumnal, or even the winter rains. And hence, also, even with us, the occurrence of a single rainy day or week, in the midst of the heats, will produce fevers; while the effect of this influence is such, that should there even be an entire rainy summer, and the subsequent one be hot and dry, this will be attended by an unusual production of malaria and disease.

And if I cannot detail all the various modes in which these circumstances may be modified, and how their effects may vary, it will be useful to make one remark on an error as relating to it which is universal among us, and into which even Lind has fallen. The error is, to think that the rain, the moisture, or the cold is itself the cause of the diseases which follow this state of things; while it is obviously a case analogous to that of Africa, if less severe, and the malaria is produced by these circumstances on soils which I formerly pointed out, and which Lind, like every one else, had neglected. But if I must pass over many interesting and useful conclusions to be drawn from these general principles, there is one fact which I must notice, and it is this:—

In spring, the combination of heat and moisture, easily explained, generates, most commonly, intermittents; or the effect of the malaria at this season differs from what it does in autumn: while as the heat advances and the ground dries, this kind of fever ceases to be produced, a new species, or the summer remittent, taking its place when the heat and the moisture of autumn begin to act. But under peculiar seasons of heat and moisture with us, it sometimes occurs, as it has done

within the last years, that the intermittent season runs into the remittent one, or there is no midsummer interval of freedom from disease;" while it has also happened, and in some parts of England in this last year, that what would have been intermittent fever in other years has been remittent; or the common fever has occupied the whole summer, continuously, even from March to November, as is the case in the worst regions of southern Europe.

Now, under these exceptions, which I was bound to explain, the commencement of intermittent, or of vernal ague, may be fixed about the middle or end of March, and its termination similarly in May; while that of remittent may be placed in the beginning of August, and its termination with the middle or end of October. How these periods may otherwise be affected by the more or less insalubrious nature of the district or place, will easily be judged of by those who will reflect for themselves on what I dare not explain, lest I should infringe too far on my limits. All else that I can venture on, as to this part of the question in hand, relates to the effects of the different times of the day on the production, propagation, or influence of malaria, and it is one which is of no small importance in a practical view.

Whether the changes as to temperature and moisture which occur within the space of twenty-four hours, affect the production or propagation of malaria, I will not here inquire minutely, from the fear of prolonging this very limited paper; but the general facts, as to its effects, are these: If we commence with the sun on the meridian, there appears, even in the worst climates, very little hazard of fever; while in Italy, it is believed that there is, generally, little or no hazard, except in some peculiarly pestilential places, and under particular kinds of inattention or neglect. Either the malaria is decomposed or destroyed by the heat, or else the air from its dryness ceases to be a conductor; but as evening approaches, its influence becomes powerful and dangerous, being supposed most generally to extend all through the night; while in some parts of that country it is a popular belief that it terminates before midnight, or with the precipitation of the atmospheric moisture. Whether this last opinion is true or not, the general fact explains the popular

belief, and truth, respecting the poisonous effects of dew in the hot climates; the supposed pernicious quality of this depending evidently on the malaria by which its formation is accompanied. And in this case it is probable that the evil arises, not from a fresh or peculiar generation of malaria, but from the mere fact that the moist atmosphere is a better conductor than a dry one.

Not to be unnecessarily minute, we thus also explain the danger of exposure to the morning air in similar situations; the facts, as they relate to the conducting of malaria, being the same, though the meteorological circumstances are somewhat different. Hence, also, we see why the grey mists which hang over wet grounds in the evening in our own climate, are esteemed pernicious; the truth, however, being, that they are perfectly innocent at certain seasons and in certain places—as in the greater part of Scotland, for example, or in those places and at those periods where malaria is not produced. The distinction is valuable, because of the inconvenience of restrictions on this subject, and because to know where the hazard really lies is to reduce those, and also to prevent the infraction of rules by not extending them beyond what is necessary; and thus also by seeing what are the real dangers of what is called night air, we more easily avoid them. Night air is avoided now, under a false philosophy, because it is cold or damp, or for some other vague reason; while the dangers from mere dampness or cold are as nothing compared to those here pointed out; which also occur precisely where they are least feared, namely, in warm summer evenings, after refreshing showers, and so forth. Hence it is that fevers are produced in summer, in rural situations, and especially perhaps amid the most engaging scenery, by evening walks and exposure to what is naturally considered, as it is felt to be, a balmy and refreshing sequel to a hot day. Let this be enjoyed where it can with safety, and as it often may; but such evening walks will not be safe in any of those situations which I need not repeat here, after having detailed them as I have done in the former paper. And lest I should be accused of wishing to excite unnecessary alarm, I consider, on the contrary, that it ought to be diminished by these remarks; because, if we take the whole of

England, there is perhaps not one acre in a hundred thousand where there is danger from night air, or from malaria in any mode; so that to distinguish where that lies, is to have relieved from useless fears all those who may learn to make the distinctions under review.

To pass from what relates to climate and season, and to proceed to the propagation, simply, of malaria, it is almost superfluous to say, that its influence, as to the production of disease, is much regulated by proximity, which implies a state of concentration or accumulation. Hence the danger arising from vicinity; while, as I formerly remarked, where the generating source is small, this becomes necessary to its effect, since dilution may be expected to destroy the power of the poison.

For analogous reasons, its effect in the production of disease is increased by concentration or condensation; and such a state of things takes place in narrow and confined valleys, or in places surrounded by woods, or in woods themselves; in any situation, in short, where the poison is produced, and is so sheltered from winds that ventilation becomes difficult. And if it is probable that this is one chief reason of the peculiarly insalubrious nature of woods and jungles in hot climates, so is it an universal remark in Italy, that the short valleys in which the air cannot circulate are among the most pestilential spots. And if this explains, also, in some measure, the bad effects of calm weather, so does it account for the unusually pestiferous nature of rivers and lakes confined within wood, as are those of the tropical climates, and as there are many also in different parts of Europe. That we ourselves are not exempt from these additional causes of the influence of malaria, would be easily shown by many references, were it not for the reason which has caused me to exclude them.

It is another important question for practice, how far and in what manner malaria can be conveyed by the winds to places where it is not produced, so as to act in exciting disease. That it is conveyed to certain distances by winds is amply proved by an abundant experience, and I may first detail a few of the most useful particulars as to this fact. In Italy and Greece, it is observed, that where long valleys terminate on sea shores, on which the exits of the rivers are swampy, it is an

effect of the sea breeze, by crossing such marshy ground, to convey the malaria up into the interior country, to considerable distances, and to places which are in themselves not insalubrious. Thus, also, does such a breeze, especially when it is a warm wind, convey the poison up the acclivities of hills, even to a considerable range of distance or elevation; a process facilitated by the natural tendency of such winds to ascend. And as a striking proof of this migration of malaria, it appears from Capt. Smyth's statistical account of the insalubrious villages in Sicily, that out of more than seventy, about one-half are not seated near or on lands producing this substance, but on acclivities, at varying distances—thus receiving it through migration. The same is remarked by Montfalcon of many towns in France; while in some, the place at a distance is even more unhealthy than that which is immediately situated in the marsh itself: and in our own country, this is equally said to be true of the backwater at Weymouth, and of the marshes of St. Blasey in Cornwall, acting more powerfully at some distance than in the immediate spot.

With respect to the absolute distance to which the malaria can be conveyed, it is yet an obscure circumstance, or at least the maximum has not been fixed; but it is at least ascertained that the convent of Camaldoli receives it from the Lake Agnano, at a distance of three miles; while from certain naval reports, a distance of five miles has been proved to permit its transmission,—and from an evidence that cannot be doubted, inasmuch as it was the sudden breaking out of fever in a healthy ship, anchored at that distance from the shore, on the coming off of the land wind, attended by its peculiar and well-known smell.

These facts are satisfactory thus far, and it would be abundantly easy to add to them; but there is reason to suspect that it can be conveyed to far greater distances, in certain favourable circumstances: those reasons, in the first place, being derived from certain meteorological analogies and considerations, and in the next confirmed by experience. It is notorious that the ague appears on our eastern coasts with the first east winds of spring; and while this circumstance is most common on those of England, as for example, in Kent, Essex, Norfolk,

Suffolk, and Lincolnshire, it is not thus limited, since it is known to happen further north, and even in Scotland, where malaria is not indigenous to the soil. It is very true that if we take any inland position in the places thus noted, the natural solution is, that the malaria is generated in the very soil itself of England, and merely propagated, perhaps even to very moderate distances, through those winds. But the occurrence of disease cannot be explained thus, when the place in question is so situated that there is no land to the eastward, or when the breeze is, most literally and rigidly, a sea breeze; while, when ague thus occurs on the east coast of Scotland, where it is not produced by the soil, it must be imported by the east wind.

These are the facts; while as malaria is not produced by the sea itself in any known circumstance, though a vegetating sea beach may give rise to it, we must seek the cause in lands far distant, and consider this as a case of propagation of the poison from the shores of Holland; and those shores are unquestionably competent to that effect: so that the only question that remains, the fact being admitted, is, whether, *à priori*, or theoretically, such a view is probable, or whether it is consistent with those physical principles that are concerned in the propagation of malaria.

I am aware that such a view will excite the incredulity of those who have not attended to this subject; though it appears to me that it comprises nothing averse to our knowledge of the philosophical circumstances concerned. In the first place, let us remark that the east wind, and particularly the east winds of spring, are notorious for their moisture, and that a moist air is the best conductor of malaria, as moisture in the air, under the form of evening mists, or in other modes, appears even to be its proper vehicle, or residence, if I may use such a term; and though I have not as yet separated the case of a fog, I may now remark, that the effect in question, or the production of agues by fogs arriving from the sea, is even more notorious than their generation by an ordinary clear wind. So notorious and popular, indeed, is this fact, that the fog itself is deemed the source of the disease, as the east wind under any form is, in other circumstances; while I hope it will even now appear,

that the real cause lies in the malaria transported or conveyed by those winds or fogs, and of which they are the true and best repository and vehicle.

And these are the reasons for thinking that the malaria, with the wind, may be transported to a distance as great as that which the present view requires; most easily perhaps in a fog, but without difficulty even in a clear wind. It is remarkable that the east wind, as it is the most persevering, is that one also which preserves the most steady horizontal and linear course. I have also shown, in a former work, that it is a property of winds to travel in distinct lines through a tranquil atmosphere, and often in streams of a very limited breadth; that opposing streams will also move, in absolute contact; and that even rapid streams of wind will cross each other's courses without difficulty. This proves that, in any such stream, there is a principle of self-preservation or integrity, and renders it probable that the several portions retain the same relative places to each other, at any distance, during the career of the whole: and there is a proof of this afforded in the fact of those columns or streams of insects which are brought over by such winds, and very frequently from those very countries, or from Holland and Flanders, in the most regular order, or without disturbance or dispersion.

Hence it may be argued, that if a malaria, generated any where and conveyed by the winds, can be transported to a distance of three miles, as has been proved, there is no reason why it should not travel much farther, or to any distance that can be assumed: and if this be true of a clear wind, the case of a fog is even a much stronger one; since there is little reason to doubt that the individual parts of such fog, in any assumed mass, will retain their relative places to each other, as perfectly after a journey of any given number of miles, as they did at the point of production; and if a portion of malaria has been united to a portion of fog, in the marsh which produced both, or whence both have come, there is every apparent reason why it should be found in that same portion at any farther or assumed distance, because there is no cause for either its dispersion or its decomposition.

A fog is a cloud, simply; and it is notorious that a single

cloud, and often of very small dimensions, will remain at rest in the atmosphere, or travel very many miles without the loss of its integrity; however we may imagine it assailed by the various meteorological causes of destruction, as well as by mechanical violence. This in itself proves the consistency with which a current of wind preserves the relative positions of its integral parts; because it is plain that a disturbance among these must disturb or destroy the cloud which, in reality, forms a portion of that current, as a gaseous body: and since that cloud is a mist, since it might have been the very evening mist embodying a malaria, and since it is its real vehicle and repository, it is plain that had it, or any individual cloud, contained such a portion of malaria, it must have had the power of transmitting that, and would actually have transported it to any distance to which itself might travel. Thus, it is evident, may a fog, generated in Holland, carry without difficulty to the limits of its range, or to the coast of England, that malaria which became entangled with it at its birth-place or in its passage; and thus, I have little doubt, is the fact of those agues explained, and this transportation to such distances established.

I cannot, at least, conceive any demonstration as to facts of this nature more convincing, nor anything wanting to the proof; while I may proceed to make some remarks on the east wind, and on fogs, simply, because they concern this question.

The proof that it is a malaria in the fog, and not the fog itself, which is the cause of disease, is evinced by the following fact; while it ought surely to be unnecessary to say, that if fog alone could produce such fever, water itself must be the poison: since a fog is a cloud, and its constituents, when pure, are only atmospheric air and water. No intermittents are ever produced on the western or northern shores by the sea fogs, and for the plain reason, that there is no land whence they arrive. The clouds of mountainous regions do not produce fevers, though these also are fogs; and what forms a most absolute proof of this is, that in Flanders, it is the fogs which come with a southwest wind, or the southerly winds themselves, which transport and propagate malaria and disease; while, as soon as the winds shift, and blow from the sea, the fevers dis-

appear, though those particular winds are so charged with fog, as to darken the whole country for days: and it will be found an invariable rule all over the world, that when a fog is the apparent cause of disease, or when an east wind is such, it is because these have been generated in a land of marshes, or have traversed one; and that, under other circumstances, or where no pernicious land lies in the way, they are as innocent as any other fogs and winds, and that the hazard and the suffering will arise from those, be they whatever they may, which traverse pestilential lands.

But I must defer this particular and interesting subject to another occasion, lest I make this article too long; and proceed to examine some other circumstances connected with the transportation of malaria.

First, however, I must notice one fact as to this transportation from Holland, partly because it is a necessary fact in the history of malaria, and partly because it might be used as an argument against the view which I have just given. The east winds of autumn are not supposed to bring remittents, as those of spring bring agues, though I cannot assert that this is absolutely true. Being assumed, the solution is easy. If the winds of this nature in spring are notedly moist, and thus vehicles of malaria, the case is exactly the reverse with the east winds of summer and autumn; or as the east wind may be the most moist of winds, so may it be the most dry; while it is a consequence of its extreme dryness, in fact, that it is always the very cause of our burning summers. This is the history of our last summers, and it is invariable, whether as it relates to seasons or single days; and it is plainly owing to its permitting the more ready transmission of the sun's rays. That it is the very harmattan of Africa, it is almost unnecessary to say; and as dry wind is not a conductor of malaria, as that poison is in fact decomposed or destroyed in these circumstances, daily and invariably, it is easy to see why the remittents of Holland should not be transported, like its intermittents, though even this may possibly happen under particular circumstances.

To proceed; and to the next remarkable facts connected with the propagation of malaria.—The most singular of these is its limitation, or that yet unexplained property by which it is

determined in a particular direction, or confined to a particular spot, while it is a piece of knowledge of some practical value. There is an appearance of incredibility about many of these facts, and, accordingly, they have not only been disbelieved but ridiculed, although nothing in the whole history of this substance is better established.

With respect to direction, in the first place, it is remarked in Italy, currently, that this poison will enter the lower stories of houses, particularly with open windows, when the next above escape; and hence, in many places, no one ventures to sleep on ground floors: and the truth of this was confirmed in the barracks at Jamaica by Dr. Hunter; as the cases of fever occurring among the men in the lower rooms much exceeded those which happened in the upper ones. But I am also informed, that in some places in Norfolk this peculiarity is reversed; or that there are houses where it is remarked that the ground-floors are safe, while no one can sleep in the upper stories without hazard.

That malaria may in some manner be attached to the soil is also well known by its effects, and especially in Italy. There it is remarked that it is extremely hazardous to cut down certain bushy plants which appear to entangle it, and that fevers are a frequent consequence of such carelessness. Thus, also, does fever seize on the labourers who may incautiously sit down on the ground, while they would escape in the erect posture; being thus, indeed, sometimes suddenly struck with apoplexy, which is one of the effects of this poison, or even with death.

It has similarly been observed that it is often retained in the shelter of drains, or in the ditches of fortifications; whence frequent fevers among the sentries on particular guards, when the other soldiers escape. And thus was it even proved at Malta, that it was transported from the sea-shore, and thus lodged in a dry ditch of the works at Valetta; all these facts being possibly to be explained, by supposing it possessed of a greater specific gravity than the atmosphere, or else attached to vapour thus weighty, exhibiting effects analogous to those which carbonic acid displays in the Solfatara.

But the circumstance most difficult of explanation is, that in Rome, and numerous places in Italy, and even where it is

transported from a distance by the winds, not generated on the spot, it is found, perennially, and through the whole course of successive years, to occupy certain places, and to avoid, as constantly, others quite near, and, as far as the eye can judge, equally exposed, and in all respects similar. Thus, one side of a small garden, one side of a street, or one house, will be for ever exposed to disease, or uninhabitable, when, at a few feet or yards distant, the very same places are as constantly free of danger: and thus it was found at the village of Faro, in Sicily, that all the troops of our army quartered on one side of the single street which formed it, were affected by fevers, and suffered great mortality, while those on the other remained in health.

But the most remarkable case of this nature known to me, is a domestic one, and which rests on the testimony of thousands of persons, or of the whole country, however incredible it may appear. It is, that between Chatham and Brighton, including every town and single house, and Sittingbourne among the rest, the ague affects the left hand side of the turnpike road, or the northern side, and does not touch the right side, though the road itself forms the only line of separation.

We cannot as yet conjecture the cause of this very singular circumstance or property, at least in cases of this nature; though, under certain events of this kind, there are some facts in meteorology that may offer a solution. These are the notorious ones, that a hoar frost, or a dew, will sometimes be found most accurately limited, both vertically and horizontally, by a definite line; stopping, for example, at a particular hedge, and reaching to a certain altitude on a tree: but for the other cases, we must yet wait for a period of more accurate knowledge as to this singular substance.

There is now one circumstance of importance, relating to the destruction or decomposition of malaria, which must not be passed over, from the interest of the facts depending on it: this is, that its propagation is checked by the streets of a crowded town, and apparently owing to this very cause, decomposition. Thus it is observed, that the fever never appears in the Judaicum of Rome, and, similarly, that the crowded streets and the poor people escape, when the opulent houses and open

streets are attacked; and hence the Villa Borghese, among many other palaces and opulent houses in Rome, has been abandoned, while such desertion, being limited exclusively to houses where the air is most open and free, naturally excites wonder: the cause, however, is now plain; and thus it now appears why it was that the Penitentiary in Westminster suffered formerly from dysentery, originating in this cause, when no such disease appeared among the neighbouring inhabitants.

And if this fact is of value as it may relate to the erection of open streets in any place of this nature, it is most important to point out what has been the continuous effect at Rome, as the ultimate consequences threaten to be extremely serious.

It appears that from cutting down some forests which many years ago occupied the declivities of the hills to the southward of Rome, the malaria was let in upon that city from the Pontine marshes; and, further, that the extirpation of a similar wood to the eastward had let in the same poison upon another quarter. Thus it has been found to enter the city through the Porta del Popolo, while, for many years past, it has been gradually extending its influence through the streets; leading annually and successively to the abandonment of many houses and palaces, and still annually increasing and extending its ravages; so as, at length, as I understand, to have even become sensible at the Vatican. And the lines which it follows are distinctly traced out by the inhabitants; while, as I have already said, it is only the houses of the opulent which suffer, further than as the abandonment of these may also influence the inferior ones in their neighbourhood.

Whatever the original cause may be, and however the direction, abstractedly, may be regulated by the winds and the forms of the streets, or by local and fixed circumstances, it is plain that the annual extension is the consequence of desertion, and that as the inhabitants retire from before it, it acquires the means of making a new step and a further progress; because thus they withdraw those fires and smoke, or whatever else it be, dependent on human crowds, which decomposes and destroys this substance. And hence it must follow, that as Rome shall become still further abandoned and depopulated, from want of industry, or from political feebleness

added to this cause, the effects must be expected to increase in a sort of geometrical ratio; almost leading to the fear that the whole city itself may, in time, fall a victim to it, or become abandoned to the wolves and mosquitoes.

If I dare not inquire more minutely into the remaining circumstances connected with the propagation of malaria, lest I should extend this article to an inconvenient length, it is necessary now to offer some remarks on prevention, and especially as it relates to this circumstance—the propagation of the poison; since the rules for prevention, as far as this relates to production, may be deduced from what was said in a former paper on this subject, and relate chiefly to the drainage of lands, and to other practices, more or less obvious, which a little reflection will, without much difficulty, deduce from what was there said.

It is plain, in the first place, that as far as the winds are concerned, it is by opposing obstacles to their course that we must attempt to counteract or divert their influence; and that, in this case, it is through the use of trees alone that we possess any power. Thus reversely, as in the case just stated, the cutting down of trees and forests has often been a serious cause of diseases in certain countries, by admitting a malaria to particular spots; though it is easy to see that where any given spot suffers from malaria, through condensation or confinement, the clearing away of these would be the remedy, by attaining a free ventilation. To detail the particular modes in which remedies may be applied through this species of aid, is obviously unnecessary, and not easy, as it must depend on local circumstances, differing for each place; but I may remark, as an example in illustration of my meaning, that where, as in many of the narrow and prolonged valleys of Greece, the sea shore is a marsh, the remedy would be to plant a screen of trees beyond it, and thus to prevent the sea winds from passing into the interior. And thus did the ancient Romans compel the planting of trees on the shores of Latium, to check the current from the Pontine marshes; rendering groves sacred, under heavy penalties, and enacting other laws with the same intentions.

With respect to such temporary precautions in these cases

as may concern armies in the field, or in camps, it is plain that they will depend on attention to the courses and seasons of the winds; while it would be abundantly easy to accumulate, from the histories of campaigns, the most fearful examples of mortality produced by neglect of these and similar precautions, and even down to almost the very date at which I am writing: and there can be no hesitation in saying, that an intimate and accurate knowledge of every thing which concerns the production and propagation of malaria, forms a most important branch in that information necessary to a soldier, and above all to the quarter-master-general's department and the medical staff: while, did I dare to record but a very small portion of the mortality experienced, not only in our own armies, but in those of Europe at large, during even the last war, from ignorance or neglect on this subject, it would, I believe, be found that it almost equalled the mortality produced by the actual collision of war itself. Walcheren will not soon be forgotten; if we have ceased to think of our mortal Havannah expedition; and if a French army at Naples was diminished by twenty thousand men, out of twenty-four, in four days, from this cause; if Orloff lost nearly his entire army in Paros; if Hungary has more than once destroyed ten times the number of men by fever that it did by the sword,—these are but trifles in the mass of reasons for saying, that no subject can well be more important, and no knowledge much more necessary to the commander of an army.

Some other points relating to prevention may deserve a few words of notice, before I pass from this subject; if here, also, I must be brief. Not to repeat the cautions founded on what relates to the power of evening and morning, it has been asserted that the use of a gauze veil will prevent the effect of malaria; and it is not improbable that the air accumulated within that, may have the power of decomposing the poison: it is an opinion, at least, which is universal among the people in Malta, and very general in Spain and Portugal. It is also found that fires and smoke are useful, and especially on military service; the experiment having been tried on a very large scale by Napoleon before Mantua, and on a smaller one in Africa, with the most perfect success. With respect to per-

sonal precautions, it is universally recommended to use wine and a good diet, and especially never to leave the house in the evening in situations peculiarly insalubrious, without the previous use of wine or spirits; whence the universal practice of Holland in this respect. Thus, also, narcotics prevent its influence; whence the wide use of tobacco, of which the salutary effects appear to be most amply established.

As to the tropical countries, there is here also one important remark, which, from the great neglect of the fact, and its ruinous consequences, appear particularly to demand a statement in this place. It is the universal experience of the inhabitants, that the attack of malaria, or the production of fevers, is aided by the use of a full or animal diet; by the use of some particular articles of food, such as butter; by excess in eating, generally; and, above all, by eating in the heat of the day. This is not merely well known to the negroes, but the fact is distinctly stated to travellers, and the caution urged, however often it has been neglected, and especially by our own countrymen. Of this, in particular, Major Denham is a strong testimony; while he attributes his own exclusive preservation to his having rigidly followed the recommendations of the natives, which were always urged with the greatest earnestness. And if we examine the causes of death, in most cases, of our African travellers especially, I think there will be strong reasons for believing that their lives have often been sacrificed to this very negligence or obstinacy; while it is most evident that Niebuhr's party, in particular, owed the loss of their lives to what may be safely called gluttony: and it is to be suspected that this will also explain the loss of Captain Tuckey's party; while, with respect to nations, it has long been known that the English, the Dutch, and the northern voracious people in general, who habitually indulge themselves in the customs of their original country as tropical colonists, have always been greater sufferers from the effects of those climates than the French and the Spaniards, and apparently from this very difference. And there seems little doubt, generally, that the vegetable diet of Africa and Hindostan is the best security against the evil influence of those climates, and that the chief sufferings of our

own colonists arise from transferring to those situations their ancient habits of full and free living.

As I must not prolong this subject much further, I shall now pass to a few remarks, but very brief ones, on the geography of malaria as it relates to those parts of the continent of Europe most frequented by English travellers; not daring to take room for actual and useful information on that head, but wishing to point out merely the importance of such geographical knowledge to those persons, on account of the hazards which they so universally incur from that ignorance or neglect, and of the great mass of suffering, and also of mortality, which has been the lot of persons who had resorted to those climates as travellers, or migrating residents, from various motives, and not unfrequently with views to health. How often health has been lost where it was sought, will be but too apparent to any one who has chanced to possess an extensive acquaintance of this nature.

Of Italy I can but afford to say generally, that except at a very few points where the Alps or Apennines reach the sea, the whole of its shores are pestilential, and often to such a degree as to lead to their entire desertion, more frequently to their abandonment in summer. And to avoid wet lands, or low lands, is not always a sufficient precaution; since the most pestilential parts of the maremma of Tuscany are dry, and since the annual mortality of Sienna from fevers, even without epidemics, is one in ten. In the north of Italy, the great plain is similarly insalubrious; though the more unhealthy district does not commence until we arrive at Mantua, extending thence to the sea. Of the Mediterranean islands, I can only afford room to say, that the same rule holds good as to the sea coasts, while the entire of Greece in the same circumstances is similarly unhealthy, and subject to autumnal fevers in as great a degree as the worst parts of Italy. The same is true of Spain and Portugal, and the same rule also will be a guide; namely, that malaria is to be expected in all the flat grounds, even when under cultivation, and at all the exits of rivers on the sea, even though no marshes should be present: and if I were desirous to name any tract of land in Spain peculiarly insalu-

brious, it would be the province of Valencia; while Carthagena is almost invariably fatal even to those who, as labourers, are compelled to resort to it for the needful work of its port, even during a few days.

Of France, little as it has hitherto been suspected by those who, associating the term malaria with Italy, have been accustomed to consider it as peculiar to that country, it would scarcely be untrue to say that it contains as large a portion of insalubrious territory as Italy itself, and produces fever and disease of as great severity and extent, not merely on its sea coasts, but over very extensive tracts in its interior. And this insalubrity may be conjectured, when there are entire districts in which the average of life does not exceed twenty, and in which the entire people are diseased from their births to their graves. Such tracts are found chiefly on the course of the Loire, and some other of the great rivers; and among them, Bresse in the Lyonnais, the plain of Forez, and Sologne in the Orleannais, are of the most notorious; while the coasts of Normandy, and the whole of low Britanny, are similarly subject to eternal intermittents, or to epidemic seasons of autumnal fevers, amounting to absolute pestilences. And how English families have suffered in this country from the incautious choice of residences in such places, will be easily ascertained by whoever shall be at the trouble of making the necessary inquiries.

But as I dare not pursue this extensive subject, I can only suggest to our countrymen the utility of making themselves acquainted with this matter, and with this dangerous geography, before encountering the hazards which await them; while to physicians I need still less name the necessity of that knowledge, since it is so often their duty to choose and recommend for their patients, and since no man can feel much at his ease who finds that he has sent into a land of malaria the patient who has already been suffering from its diseases, or that where he speculates on the cure of a consumption, that cure is attained through the death of the patient, at Avignon, or at Poitiers, or Nantes, or in some or other of the numerous places subject to this most fearful poison.

It remains only to give a brief enumeration of the diseases

which are the produce of malaria, and of the general condition of the inhabitants in the countries subject to it. With respect to this latter, the most remarkable general fact is the contracted duration of life. In England, the average may, if not very accurately, and indeed considerably under the mark, be taken at 50; and when in Holland it is but 25, it follows that the half of human life is at once cut off by this destructive agent. In the parts of France to which I have alluded, it becomes as low as 22 and 20, and Condorcet, indeed, has calculated it as low as 18. With this, very few attain the age of 50; and in appearance and strength, this term is equivalent to 80 in ordinary climates; while 40 forms the general limit of extreme and rare old age. The period of age, indeed, commences after 20; and it is remarked, in particular, that the females become old in appearance immediately after 17, and have, even at 20, the aspect of old women. In many places, even the children are diseased from their birth; while the life which is dragged on by the whole population, is a life of perpetual disease, and most frequently of inveterate and incurable intermittents, or of a constant febrile state, with debility, affections of the stomach, dropsy, and far more than I need here enumerate.

While the countenances of the people in those countries are sallow or yellow, and often livid, they are frequently so emaciated as to appear like walking spectres, though the abdomen is generally enlarged, in consequence either of visceral affections or dropsy. With these, rickets, varices, hernia, and, in females, chlorosis, together with scorbutic diseases, ulcers, and so forth, are common; and it is even to be suspected that the cretinage may depend on this cause, since goitre is also one of the results of malaria, and since, in the Maremma of Tuscany, idiotism is a noted consequence of this pestilential influence.

The general mental condition is no less remarkable; since it consists in an universal apathy, recklessness, indolence, and melancholy, added to a fatalism which prevents them from even desiring to better their condition, or to avoid such portion of the evils around them as care and attention might diminish: and while it is asserted that even the moral character becomes

similarly depraved, I prefer a reference to Montfalcon for a picture which it would not be very agreeable to transcribe.

As to the absolute or positive diseases, besides those which I have already named, I need scarcely say that remittent and intermittent fevers, under endless varieties and types, form the great mass; and next in order to them, may be placed dysentery and cholera, together with diarrhœa. To these I must also add, those painful diseases of the nerves, of which sciatica stands foremost, and the remainder of which may be ranked under the general term of neuralgia; and further, a considerable number of inflammatory diseases of a more or less remittent type, among which rheumatism under various forms is the most general, and the intermittent ophthalmia the most remarkable. Lastly, I must include the various paralytic affections; since apoplexy is one of the primary and direct consequences of malaria, as various paralytic affections are the produce of intermittent, or the consequences of the diseases of the nerves which are associated with it.

It is still a curious and interesting fact, that this poison affects, in an analogous manner, many different animals, and appears, in reality, to be the cause of all the noted endemics and remarkable epidemics which occur in the agricultural animals in particular. This has been noticed even by Livy: and in France and Italy it is equally familiar that the severe seasons of fever among the people are similarly seasons of epidemics to black-cattle and sheep, while the symptoms are as nearly the same as they could be in the circumstances, and the appearances on dissection also correspond. Thus also does it appear probable, that the rot in sheep is actually the produce of malaria, as is indeed the received opinion among French veterinarians; while Mr. Royston has observed that the animals of this class are subject to distinct intermittents.

And while it is not less familiar in the West Indies, and in Dominica particularly, that dogs suffer from a mortal fever in the same seasons and periods as the people, the epidemic always breaking out in them first, I have the most unexceptionable medical evidence of the occurrence of a regular and well-marked tertian in a dog; that evidence consisting in the concurring decision of many surgeons, by whom the case was

frequently examined, during a very long period. But it is time to terminate a paper, which, if it is but a sketch of an important subject, will at least convey to those to whom malaria has not hitherto been an object of attention, a general notion of the leading particulars which appertain to its natural history.

J. M.

Elements of Chemistry, including the recent Discoveries and Doctrines of the Science. By Edward Turner, M.D., F.R.S.E., &c., &c. *Edinburgh*, 1827.

This is a closely-printed octavo of 700 pages, and presents us with something more original, clear, and accurate than we have lately met with in modern chemistry. It comprehends a perspicuous view of the present state of chemical science; and, as far as its limits admit, the theoretical parts are, with some exceptions, well and distinctly worked out; nor are the practical details of manipulation neglected, though they evidently occupy a secondary place in our author's estimation. To the arrangement we must at once decidedly object—it is indeed evident that Dr. Turner has pitched upon Dr. Thomas Thomson as his *magnus Apollo*, and here and elsewhere the book is tainted accordingly.

This work is divided into four principal parts;—the first relates to what Dr. Turner, following his prototype, Dr. Thomson, calls *imponderables*, and a definition of them follows, which leads us to suggest the term *inexpressibles*, as equally appropriate. But, waiving this objection, the details relating to them are well and clearly given. Thus, after some prefatory remarks upon the subject of caloric or heat, (we prefer the latter term, and cannot allow its ambiguity,) its modes of communication are considered, first, as being *conducted* through bodies, and then as *radiating* through free space. In regard to the theories affecting the latter, our author wisely, as we think, prefers that of Prevost to that of Pictet. The *effects* of heat are next discussed, such as *expansion*, including an account of the thermometer, and of the relative capacities of bodies for heat; *liquefaction*, *vaporisation*, *ebullition*, *evaporation*, and the *constitution of gases*; and lastly, the sources of heat are mentioned, but the details are referred to other parts of the work.

Light is next treated of, but we think too hastily, and too much in the abstract.

Now the subjects of heat and light are obviously of the utmost importance to the chemical philosopher, and they are very extensive, and intricate and difficult to treat of, inasmuch as the writer is necessarily upon the confines of chemical and mechanical philosophy, and should be expert in both. When, therefore, elementary works on chemistry are so written and arranged as to serve as text-books for lectures, and indexes of reference to more accurate information, we can make due allowance for brevity; but when the subject is intended to be formally and completely developed to the student, independent of other ocular and oral aids, much more extensive description and detailed explanation is required, than is to be found either in our author's "Elements," or in any other analogous condensation of chemistry. Dr. Henry understands the requisite mode of conveying information in these cases better than most writers; and when he takes pains, and speaks for himself, has the talent of being brief, and at the same time minute, deep, and clear. Dr. Ure, as his dictionary shows, is an eminent example of such a writer—*he* of course is neglected, where, as with our author, Dr. Thomson is in the ascendant; but the article caloric, in his dictionary, will at once explain and illustrate our meaning, and would furnish an admirable foundation for a detailed essay or treatise upon the subject. So extensive, indeed, are the precincts of chemistry now becoming, that either our *systems* must become very voluminous, or we must adopt the plan, which to us appears preferable, of distinct treatises upon different branches of the science. Thus, a separate work on heat and light; another on electricity and magnetism; another on attraction and the theory of combination; a fourth on the constitution and properties of the unmetallic elementary bodies; a fifth on the metals and their compounds; a sixth on vegetable, and a seventh on animal chemistry and physiology; an eighth on the chemistry of the arts; and lastly, a treatise on chemical manipulation in general, would include all that appears essentially requisite; and as no one is supposed to be equally well versed in all branches of the science, or in all details of the art, an opportunity of selection would thus be afforded, so that each writer might choose that particular department which he is most accurately acquainted with, or which has formed his favourite study. Mr. Faraday has already, as may be said, led the way in such a plan, by the publication of his *Chemi-*

cal Manipulation, a work hitherto exceedingly wanted in the laboratory, equally useful to the proficient and to the student, and eminently creditable to the industry and skill of the author, and to the school whence it emanates. We shall of course take an early opportunity of introducing this book in a more formal way to the attention of our chemical readers.

In looking over Dr. Turner's first and second sections on caloric and light, in the *Elements* now before us, we find little but brevity to complain of;—there are, however, one or two trifling historical inaccuracies: thus, at page 14, the discovery of *invisible* heating rays is ascribed to Saussure and Pictet; but it is, in fact, of much more remote origin—it was well known to the Florentine academicians, and we may even trace the idea in Lucretius, (*De Rerum Naturâ*, lib. v. l. 609.)

> Forsitan et rosea Sol alte lampade lucens
> Possideat multum *cæcis fervoribus ignem*
> Circum se, *nullo qui sit fulgore notatus*, &c.

At page 31 we have an account of Wedgwood's pyrometer, which is said to be "little employed at present, because its indications cannot be relied on;"—the fact is, that it is never used, and that we owe to Sir James Hall ample reasons for placing no confidence in it.

The subject of *specific heat* is clearly explained, and so are the phenomena of liquefaction and evaporation. In regard to the constitution of gases, the author remarks, that the experiments of Sir H. Davy and Mr. Faraday on the liquefaction of gaseous substances, appear to justify the opinion that gases are merely the vapours of extremely volatile liquids. Mr. Faraday has proved this in regard to several of the gases, and analogy leads us to apply it to the rest;—but what share Sir H. Davy had in the discovery, we know not; for Mr. Faraday actually condensed chlorine into a liquid before Sir H. had heard or thought about the matter. *Light*, and its phenomena, as connected with chemistry, is superficially passed over in the second section, and the third brings us to the important article "Electricity."

We are willing to admit that the subject of electricity is a very difficult one for the chemist to deal with—he must necessarily say much upon it, and is equally obliged to omit abstract details which are often necessary to its explanation, and yet too prolix and bulky for an elementary chemical work. So that it requires considerable acquaintance with the subject to give a perspicuous and yet concise abstract,

such as may be useful to the student. Dr. Turner has not been very successful in effecting this *desideratum,* and has unnecessarily introduced two sections, the one on electricity, the other on galvanism. He also talks of the "science of galvanism," which is in bad taste, and erroneously asserts that the energy of the pile is proportional to the degree of chemical action which takes place; a statement by no means correct, inasmuch as the energy of De Luc's column is directly proportional to the number of alternations, and appears entirely independent of chemical action; and again, a series of 2000 plates, arranged in the usual Voltaic apparatus, when perfectly bright and clean, and the cells filled with distilled water only, give a much more powerful shock, and cause a greater divergence of the leaves of the electrometer than when the apparatus is charged with diluted acids. Here, those very singular phenomena, which electricians distinguish by the terms *quantity* and *intensity*, appear perfectly distinct; and between these our author does not sufficiently discriminate, but jumbles the whole under the term *activity*. In describing the chemical energies, too, of the pile, or its decomposing powers, the Doctor entirely overlooks the important and curious influence of water. He says that acids and salts are all decomposed, without exception, one of their elements appearing at one side of the battery, and the other at its opposite extremity; (*i. e.* we presume, at its positive and negative poles.) But the fact is, that, excepting where it merely acts as a source of heat, nothing is decomposable by electricity without the intervention of water; the hydrogen and oxygen of which respectively accompany the elements of the other compounds. Not an atom of potassium can be obtained unless the potassa be moistened; nor can any salt be decomposed except water be present. Sir Humphry says, it is required, to render the substance a conductor; but its operation is more recondite, and there is something mysterious and still unexplained in the uniform appearance of hydrogen and oxygen at the opposite poles, when far apart in water, and in all other cases of true polar electro-chemical decomposition. At page 86, the unfortunate protectors of ships' bottoms are introduced—a subject about which the less is said the better;—and, as to electro-magnetism, it is merely mentioned as to its leading phenomena, in the space of three or four pages; nor is anything new suggested upon the "Theory of the Pile," as it is called, which concludes the subject, and which is dismissed in the brief limit of a page and a half.

The second part of Dr. Turner's work is said to comprise "Inorganic Chemistry," and therefore embraces a very extensive field of inquiry. To the *arrangement* we have already objected; and many of the typographical and verbal errors that occur, have been noticed in a contemporary Journal, so that we shall chiefly attend to the details of the sections.

Under the head "Affinity," some of the leading facts and doctrines of chemical attraction are perspicuously set forth; but we could have wished that a variety of exploded opinions and erroneous notions had been altogether passed over, as they occupy space which might have been better employed, and can never prove of any other use o the student than to show him the errors and fallacies to which acute philosophers are sometimes liable. Of this kind, especially, are Berthollet's notions upon the subject of affinity. The doctrine of definite proportion is, on the whole, well and clearly explained; but it would have been much better and clearer, had Dr. Turner confined himself to facts, and meddled less with opinions concerning their cause; he is moreover, in many respects, historically inaccurate. He ascribes much to Dalton that honestly belongs to Higgins;—is much too merciful to Berzelius and his CANONS; and lenient beyond all endurance to the plagiarisms of "Dr. Thomson's admirable Treatise on the first Principles of Chemistry."

In the third and following sections, the simple non-metallic substances are described in an order of arrangement which must be very perplexing to the student; otherwise the details are well given, except that here and there the line between theory and fact is not sufficiently marked. Thus we are told that "hydrogen is exactly 16 times lighter than oxygen, and *therefore* that 100 cubic inches *must* weigh $\frac{33.888}{16}$, or 2.118. Its specific gravity is consequently 0.0694, as stated some years ago by Dr. Prout." Now this is a theoretical deduction founded upon the specific gravity and constitution of ammonia, (and not upon the composition of water,) and probably correct as applied to *pure* hydrogen;—but if we weigh the gas, as usually obtained, even with the utmost caution, and of the utmost purity, we shall never procure it so light as here stated, notwithstanding all the learning and argument that our worthy friend, Dr. Thomas Thomson, has issued upon the subject in his various essays in the Annals, and in his *magnum opus*. We also object to the stress which is often laid upon the whims of individuals, and upon

exploded opinions; instances of which will occur to the reader under the subject of the composition of nitrogen, and the constitution of the atmosphere. We further caution our author against admitting hints, allusions, and inuendos as to the possibility of future inventions and discoveries, as claims upon the merits of such discoveries, when they are actually made. Berzelius has talked a vast deal of nonsense about the composition of nitrogen; and should that discovery ever be made, he will doubtlessly assume the credit of having suggested the steps which led to it. Some foolish persons are apt to think that the Marquis of Worcester was the inventor of Watt's steam-engine, because he said he had means of raising water by steam, in his *Century of Inventions;* and we have heard that an eminent chemist of the present day considers himself entitled to all the merit that may belong to Mr. Brunel's carbonic acid engine, because he had previously stated the possibility of such an application of Mr. Faraday's important discoveries. The fact is, that these are woeful days for science; all the good feeling and free communication that used to exist among its active cultivators in this country, has given way to petty jealousies and quibbling scandal; one person is exalted for the purpose of depreciating another; and those causes of disgust, which some years ago induced one of our most amiable and able men of science to quit the field, and even leave the country, are becoming daily more prevalent. Were it not an invidious task, we could easily explain and unfold the sources of all this mischief, and shall indeed feel it our duty so to do, should not matters in due time take a more favourable turn; but the task is at once serious and disagreeable, and we therefore postpone it, in the hope of more favourable events. We really believe that, had it not been for the scientific conversationes held during the last seasonat the houses of a few private gentlemen connected with the learned societies, and more especially the weekly meetings at the Royal Institution, which kept up a friendly intercourse among those who were willing to profit by it, that the whole scientific world would have been at loggerheads, and in that state of anarchy of which the evils may be learned by a short residence at a "northern seat of learning."

The main object of this digression is to deprecate *party* in science; and we were led to it by observing, or thinking that we observe, something of such a tendency in the writer whose book is before us—we hope we are mistaken.

The next section comprises "the compounds of the simple

non-metallic acidifiable combustibles with each other." It includes the important subject of ammonia, of the varieties of carburetted hydrogen, sulphuretted and phosphuretted hydrogen, and cyanogen and its compounds. The metals are then treated of, and to these succeed their salts; and though the execution of this part of the work betrays some haste, it shows also considerable reading, and some originality: the general views are well and clearly sketched; but there are many points upon which we are entirely at variance with our author; and we more especially object to his account of the action of chlorides upon water, and to his notions concerning the "muriates of oxides," a class of compounds of which, with one or two exceptions, we are disinclined to admit the existence. If common salt be a *chloride* of *sodium*, and experiment obliges us so to regard it, what is there in its aqueous solution that should lead us to consider it as containing a *muriate* of *soda*; what evidence of any new arrangement of elements? Dr. T. is certainly in mistake, when he says, "for all practical purposes, therefore, the solution of a metallic chloride in water may be viewed as the muriate of an oxide, and on this account I shall always regard it as such in the present treatise." This inconsiderate dogma taints much of the reasoning upon the chlorides, &c., and is manifestly culled in the Thomsonian school, though we have indeed heard that a Professor at Edinburgh thus addresses his pupils upon the above subject: "The elaborate researches of the illustrious Davy have taught us that common salt is a binary compound of chlorine and sodium, a chloride, therefore, or a chlorurct of sodium. But it is only chloride of sodium whilst quiescent in the salt-cellar; for no sooner does it come into contact with the salivary humidity of the fauces, than, by the play of affinities, which I have elsewhere explained, the sodium becomes soda, and the chlorine generates muriatic acid;—that, therefore, which upon the table is chloride of sodium, is muriate of soda in the mouth; and this again, when desiccated or deprived of humidity, retrogrades into its former state."

Dr. Turner again falls into error, as we humbly conceive, in calling certain salts, such, for instance, as those of the peroxide of iron, *sesquisalts*, a term properly applied in those cases only where one proportional of a protoxide unites with one and a half of an acid, such for instance as the *sesquicarbonate of soda*, &c., but in the sesquisulphate of iron, one proportional of the peroxide contains 1.5 of oxygen, and ne-

cessarily, therefore, (according to Berzelius' canon, if the Doctor pleases,) requires 1.5 of acid to convert it into a salt; just as the commonly constituted peroxides (containing two proportionals of oxygen) require two of acid. Dr. Thomson, with all his nomenclatural pretensions, has fallen into the same error.

The part of our author's work which treats of the chemistry of organic bodies is, upon the whole, an unexceptionable and accurate epitome of that complicated branch of the science. It has its inaccuracies, but they apparently arise out of the difficulty of condensing into the space of a few pages, matter which, as we have elsewhere remarked, would require an ample volume for its extended and perspicuous details.

In our hasty account of this work, we have rather dwelt upon its defects than its merits, in the hope of seeing another and more extended edition, free from what we consider as serious obstacles to the success and usefulness of the present production. We hope that Dr. Turner will not feel offended at the freedom with which our remarks are offered. We are anxious that a writer of such good information should be induced to think for himself; at least, that he should accurately weigh the pretensions, and inquire into the originality of those views and researches upon which he bestows such unqualified and, in our opinion, undeserved praise, and to which he assents with a facility unbecoming one who evidently possesses the means of testing their merits.

Experiments on Audition.

[Communicated by Mr. C. Wheatstone.]

The recent valuable experiments of Savart * and of Dr. Wollaston have added to our stock of information several important and hitherto unnoticed phenomena relating to audition; but, notwithstanding the investigations of these distinguished experimentalists, and though the physiology of the ear has been an object of unceasing attention for many centuries, yet we are far from possessing a perfect knowledge of the functions of the various parts of this organ. The description of new facts illustrative of this subject cannot, therefore, be devoid of interest;

* Recherches sur les usages de la membrane du tympan et de l'oreille externe; par M. Felix Savart. *Annales de Chimie,* tom. xxvi. p. 1.

and though I do not anticipate that the observations contained in this communication will lead to any important results, their novelty may claim for them some attention from the readers of your Journal.

§ 1.

If the hand be placed so as to cover the ear, or if the entrance of the meatus auditorius be closed by the finger without pressure, the perception of external sounds will be considerably diminished, but the sounds of the voice produced internally will be greatly augmented: the pronunciation of those vowels in which the cavity of the mouth is the most closed, as *e ou*, &c., produce the strongest effect; on articulating smartly the syllables *te* and *kew*, the sound will be painfully loud.

Placing the conducting stem of a sounding tuning-fork* on any part of the head, when the ears are closed as above described, a similar augmentation of sound will be observed. When one ear remains open, the sound will always be referred to the closed ear, but when both ears are closed, the sound will appear louder in that ear the nearer to which it is produced. If, therefore, the tuning-fork be applied above the temporal bone near either ear, it will be apparently heard by that ear to which it is adjacent; but on removing the hand from this ear (although the fork remains in the same situation) the sound will appear to be referred immediately to the opposite ear.

In the case of the vocal articulations, the augmentation is accompanied by a reedy sound, occasioned by the strong agitations of the tympanum. When the air in the meatus is compressed against this membrane by pressing the hand *close* to the ear, or when the eustachian tube is exhausted by the means indicated by Dr. Wollaston, the reedy sound is no longer heard, and the augmentation is considerably diminished. The ringing

* The tuning-fork consists of a four-sided metallic rod, bent so as to form two equal and parallel branches, having a stem connected with the lower curved part of the rod, and contained within the plane of the two branches. The branches are caused to vibrate by striking one end against a hard body, whilst the stem is held in the hand. The sound produced by this instrument when insulated is very weak, and can only be distinctly heard when its branches are brought close to the ear; but instantly its stem is connected with any surface capable of vibrating, a great augmentation of sound ensues from the communicated vibrations. The facility of its insulation and communication renders it a very convenient instrument for a variety of acoustical experiments.

noise which simultaneously accompanies a very intense sound, proceeds from the same cause, and may be prevented by the same means. This ringing may be produced by applying the stem of a sounding tuning-fork to the hand when covering the ear, or by whistling when a hearing trumpet is placed to the ear. As a proof that the resulting augmentation, which, when great, excites the vibrations of the tympanum, is owing to the reciprocation of the vibrations by the air contained within the closed cavity, it may be mentioned, that when the entrance of the meatus is closed by a fibrous substance, as wool, &c., no increase is obtained.

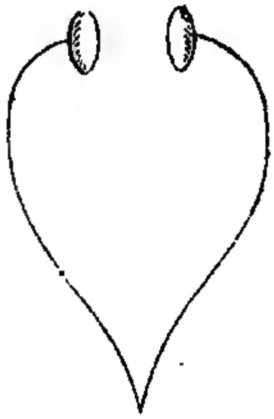
If the meatus and the concha of one ear be filled with water, the sounds above-mentioned will be referred to the cavity containing the water in the same way as when it contained air, and was closed by the hand; it will be indifferent whether any partition be interposed between the cavity and the external air, as the water is equally well insulated by a surface of air as by a solid body.

§ 2.

The preceding experiments have shown, that sounds immediately communicated to the closed meatus externus are very greatly augmented; and it is an obvious inference, that if *external* sounds can be communicated, so as to act on the cavity in a similar manner, they must receive a corresponding augmentation. The great intensity with which sound is transmitted by solid rods, at the same time that its diffusion is prevented, affords a ready means of effecting this purpose, and of constructing an instrument, which, from its rendering audible the weakest sounds, may with propriety be named a Microphone.

Procure two flat pieces of plated metal, each sufficiently large to cover the external ear, to the form also of which they may be adapted; on the outside of each plate directly opposite the meatus, rivet a rod of iron or brass wire about 16 inches in length, and one-eighth of an inch in diameter, and fasten the two rods together at their unfixed extremities, so as to meet in a single point. The rods must be so curved, that when the plates are applied to the ears, each rod may at one end be perpendicularly inserted into its corresponding plate, and at the other end may meet before the head in the plane of the mesial

line. The spring of the rods will be sufficient to fix the plates



to the ears, but for greater security ribands may be attached to each rod near its insertion in the plate, and be tied behind the head.

A more simple instrument may be constructed to be applied to one ear only, by inserting a straight rod perpendicularly into a similar plate to those described above.

The Microphone is calculated only for hearing sounds when it is in immediate contact with sonorous bodies; when they are diffused by their transmission through the air, this instrument will not afford the slightest assistance.

It is not my intention in this place to detail all the various experiments which may be made with this instrument, a few will suffice to enable the experimenter to vary them at his pleasure.

1. If a bell be rung in a vessel of water, and the point of the microphone be placed in the water at different distances from the bell, the differences of intensity will be very sensible. 2. If the point of the microphone be applied to the sides of a vessel containing a boiling liquid, or if it be placed in the liquid itself, the various sounds which are rendered may be heard very distinctly. 3. The instrument affords a means of ascertaining, with considerable accuracy, the points of a sonorous body at which the intensity of vibration is the greatest or least; thus, placing its point on different parts of the sounding board of a violin or guitar, whilst one of its strings is in vibration, the points of greatest and least vibration are easily distinguished. 4. If the stem of a sounding tuning-fork be brought in contact with any part of the microphone, and at the same time a musical sound be produced by the voice, the most uninitiated ear

will be able to perceive the consonance or dissonance of the two sounds; the roughness of discords, and the beatings of imperfect consonances, are thereby rendered so extremely disagreeable, and form so evident a contrast to the agreeable harmony and smoothness of two perfectly consonant sounds, that it is impossible that they can be confounded.

§ 3.

Apply the broad sides of two sounding tuning-forks, both being unisons, to the same ear; on removing one fork to the opposite ear, allowing the other to remain, the sensation will be considerably augmented.

It is well known, that when two consonant sounds are heard together, a third sound results from the coincidences of their vibrations; and that this third sound, which is called the grave harmonic, is always equal to unity, when the two primitive sounds are represented by the lowest integral numbers. This being premised, select two tuning-forks, the sounds of which differ by any consonant interval excepting the octave; place the broad sides of their branches, while in vibration, close to one ear, in such a manner that they shall nearly touch at the acoustic axis, the resulting grave harmonic will then be strongly audible, combined with the two other sounds; place afterwards one fork to each ear, and the consonance will be heard much richer in volume, but no audible indications whatever of the third sound will be perceived.

§ 4.

Very acute sounds, such as the chirping of the gryllus campestris, &c., are rendered inaudible by exhausting the air from the Eustachian tube, and thereby producing a tension of the membrane of the tympanum; the different thicknesses or tensions of this membrane may therefore occasion that diversity of the limits of audibility, with regard to the acute sounds which Dr. Wollaston has pointed out as existing in different individuals; if so, it would be desirable to ascertain this limit in individuals in whom the tympanum is perforated, or destroyed.

§ 5.

When the auricula is brought forward, all *acute* sounds are rendered much more intense, but no sensible difference is per-

ceived with regard to the grave sounds. The *higher* tones of glass staccados, or of an octave flute, the ticking of a watch, all kinds of sibilant sounds, &c. are thus greatly augmented: the experiment is easily tried, by whistling very shrill notes. A still greater augmentation of the acute sounds is obtained, by placing the hands formed into a concave behind the ears, and by bending downwards the upper part of the auricula, so as to obtain a more complete cavity.

§ 6.

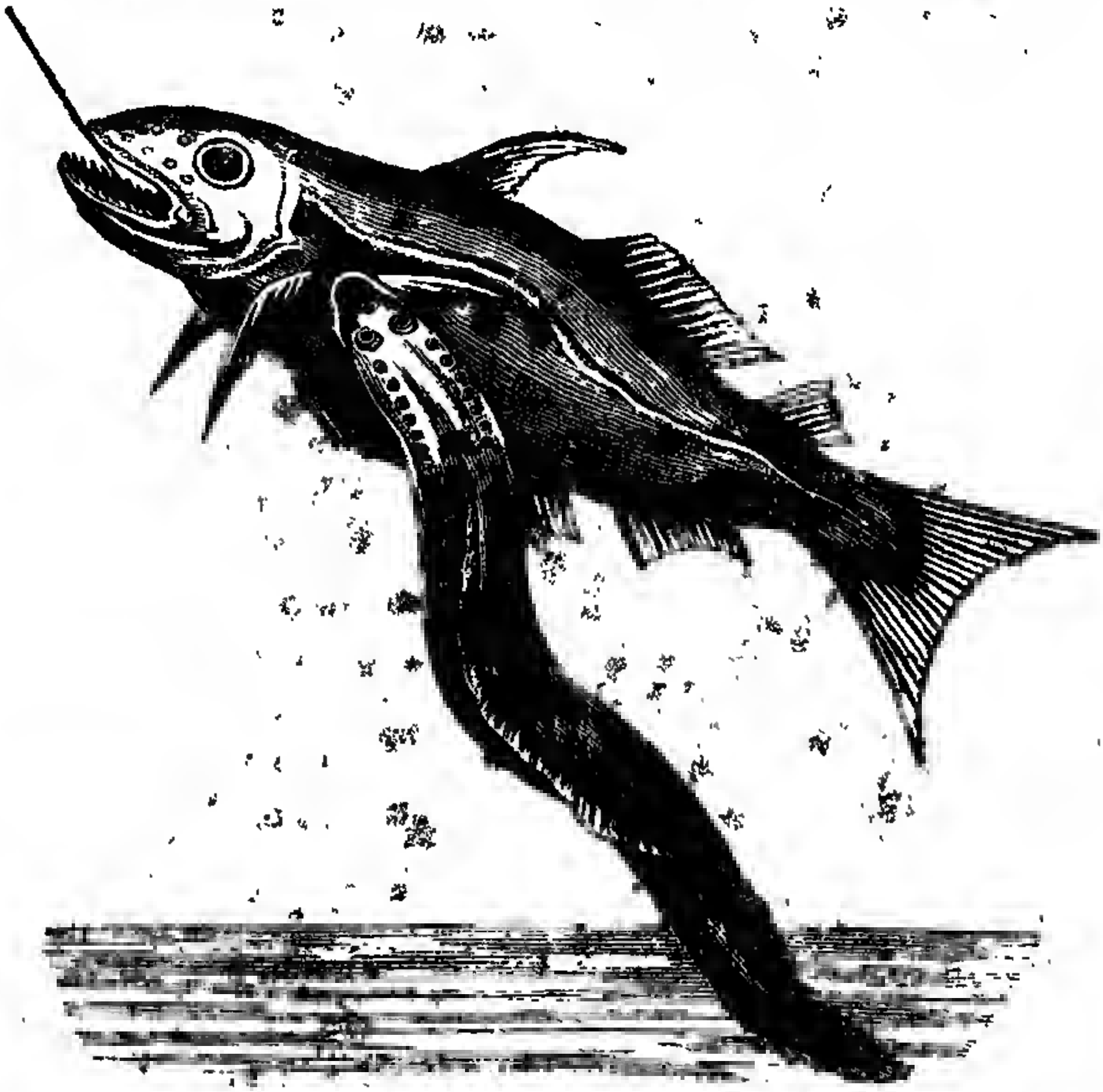
I will conclude with the following observation: I had, in consequence of a cold, a very slight pain in my left ear; on sounding the regular notes of the piano-forte, C^3 and C^4 were much louder than the others, and the loudness was much increased, by placing the hand in the manner above described to the left ear. When it was pressed close, or when the Eustachian tube was closed, the intensities of all the notes were equalized. I attribute this affection to the diminished tension of the membrana tympani, which was again increased by the operation described.

On the Petromyzon Marinus.

On entering the harbour of Dublin a few weeks ago, we were becalmed off the Hill of Howth, and to pass the tedious time until a breeze sprung up, we found some lines on board, and began to fish from the quarter-deck. We caught a number of grey gurnet; but our attention was particularly attracted by a pull of uncommon force on one of the lines. Having rendered assistance to the person who held it, we were all astonished to see rise out of the water a large fish, with apparently a double body, which, after floundering on the surface of the water, we pulled on deck. On examining this phenomenon for a short time, we were again surprized to see it separate into two parts; and then found that there were *two* large fish taken up on the same hook, the head of one having been buried under the throat of the other, to which it had firmly attached itself. When separated by force, it wriggled about on the deck with extraordinary strength and agility, and again darted on its prey, to which

it adhered so firmly, that it required very considerable exertion to detach it; for it suffered itself to be raised up by the tail and shaken, still holding the other fish suspended from its jaws. When finally separated, it showed great ferocity, darting at every thing near it, and at last seizing the deck, which it held very fast, writhing with its tail and body as if in the act of tearing it to pieces. When detached, its teeth left a deep circular impression on the wood, the fibres of which were drawn into the cavity of its jaws, so as to be raised up in the form of a cone. I now directed, that it should be put into a bucket of sea water, in the hope of preserving it alive until we arrived in Dublin, but it died in a shorter time than could be expected, from the energy and activity it had displayed, long after the other fish was dead. We had handled it very roughly, and so perhaps had mortally hurt an animal otherwise very tenacious of life.

On examining the fishes, I found that which had taken the



hook, was the *gadus Polachius*, or whiting Pollack. It was about two feet long, and it is probable its active enemy had fastened

on it after it had been hooked; if *before*, it would indicate an extraordinary insensibility to pain in an animal that could attend to the calls of appetite, whilst another was preying on its vitals. The fish which had fastened on the pollack, was the *petromyzon marinus*, or sea lamprey. It was nearly three feet long, and resembled a large eel in shape. Its general colour was a dull brownish olive variegated with bluish blotches; the back darker, and the belly paler, inclining to yellow. The eyes were small, and the mouth large and oval; but when distended, circular. The inside of the jaws was deeply concave, and studded with circular rows of sharp triangular teeth, that issued from corresponding orange-coloured papular protuberances, which formed the gums; the tongue was short and crescent-shaped, furnished with a row of very small teeth round the edge. On the top of the head was a small orifice, or spout-hole, from whence it discharged the superfluous water taken at the mouth. But the circumstance that more particularly distinguished it, was that which gave rise to the vulgar error that it had sixteen eyes. On either side of the neck, commencing just below the real eyes, was a row of seven equidistant spiracles exactly resembling eyes; they are, however, holes lined with a red membrane, and all opening into the mouth, an apparatus to supply the place of gills, whose functions are to extract oxygen from the water, and so perform the office of lungs in aquatic animals. It had two dorsal fins, one on the lower part of the back, narrow, with a roundish outline; the other commencing where the first terminated. The spine was cartilaginous, without processes. The pericardium, containing a small heart, was a remarkably strong membrane, and the liver was as green as grass.

This fish is not uncommon in the North Seas, though it most abounds in the Mediterranean, where, from earliest times, it was esteemed a luxurious dish. Fish-ponds were purposely constructed to preserve it. On our coast, Pennant observes, that it is found most frequently at the mouth of the Severn, which river it sometimes ascends, where it is occasionally taken, firmly attached to a stone by its mouth, while its tail and body are waving freely to the current. Its adhesion at such times is so strong, that it may be lifted with a stone of twelve pounds weight appended to its mouth. This faculty is owing to its

power of suction; while the circumstance of its circular jaws coming in close contact with the surface of the body excludes the external air within the cavity of the mouth, and so adheres like the hand placed on the cup of an air-pump. It is from this remarkable property, that its scientific name has been imposed *. Its vulgar name, lamprey, from lampetra, has a similar derivation. By the Romans it was named muræna. As this fish was well known and highly prized by the ancients, there is none that has been so frequently described and alluded to. Aristotle, Pliny, Tacitus, Columella, Ælian, Seneca, and Oppian, have mentioned its properties and habits, which correspond exactly with those I have described above. Pliny says, in the northern parts of France, and consequently contiguous to the British Isles, the lampreys have seven spots in the jaws, resembling the constellation of the plough, evidently the same as the eyes, which vulgar opinion assigns to the fish †. Their extreme voracity was such, that criminals were thrown among them to be devoured. Seneca relates, that Vedius Pollio, a Roman knight, ordered his servant, who had broken a crystal vase, to be thrown into a large pond of lampreys ‡; and Columella writes, that they were sometimes seized with a rabid fury; that resembled canine madness; in the access of which, they seized upon other fish, so that it was impossible to keep them in the same pond §; and to account for this extraordinary ferocity, Oppian and others assert, that the lamprey is impregnated by a serpent; the one issuing from the sea, and the other rushing down to the rocks, inflamed with madness, to consummate the impregnation; and adds, that the extraordinary intercourse was effected by the lamprey seizing the serpent's head in its

* Petromyzon; *a πέτρον*, saxum, and *μύζω*, sugere.

† In Gallia septentrionale murænis omnibus dextra in maxilla septenæ maculæ ad formam septentrionis aureo colore fulgent.

PLIN. *Hist. Nat.* lib. ix. cap. 39.

‡ Fregerat unus ex servis crystallinum ejus; rapi eum Vedius jussit, nec vulgari quadam morte periturum, murænis objici jubebatur quas ingens piscina continebat.—SENECA *de Irâ*, lib. [illegible] cap. 40.

§ Commisceri eas cum alterius notæ piscibus non placet, quasi rabie vexantur quod huic generi velut canino solet accidere. Sævitia persequuntur squamosos plurimosque mandendo consumunt.

COLUMELLA *de Re Rusticâ*, lib. ix. cap. 17.

mouth*. This singular copulation was the reason why the Romans, who were immoderately fond of lampreys, did not wish to eat them, when impregnated by the supposed serpent. Horace, therefore, makes Nasidienus, among the blunders of his supper, serve it in that state †.

Lampreys were a favourite dish with our own early monarchs. Henry II. died by eating them to excess. The celebrated Pope also owed his death to a surfeit of them. Doctor Johnson remarks in his life of the poet, that he was in the habit of cooking them himself in a silver saucepan. The Corporation of Oxford still make up a periodical pye of this fish for the king, in compliance with ancient usage. But lampreys have lost their rank at corporation feasts, in consequence of the more delicious and wholesome turtle being introduced into modern cookery.

I have never noticed lampreys in the Dublin fish-market; and though they are frequently used in the South of Ireland, I do not know if they have ever been made an article of food in Dublin, or the north, where they are rarely met with.

C.

Observations upon the Motion of the Leaves of the Mimosa Pudica.

[To the Editor of the Quarterly Journal of Science.]

Dear Sir,

TOWARDS the latter part of this summer, Mr. Gilbert Burnett and myself made several experiments with a view to ascertain the nature of the movements exhibited by the sensitive plant. We afterwards found that the greater part of the facts which we had observed, had been previously described by Mr. Lind-

* Αμφὶ δε μ[illegible] φα[illegible]ρχεται ουκ αιδ[illegible]ον
Ὡς μεν γαμου τ[illegible] ἐξ ἁλος ερχεται αυ[illegible]
Προφρων ἱμ[illegible] ἱμειροντι γαμοιο[illegible]
Ητοι ὁ μὲν φλογερη τεθοωμενος [illegible] λυσση
Μαινεται [illegible]λοτητα και ἐγγυθι συρεται ἀκτῆς
Πικρος ὄφις. κ.τ.λ.—OPPIAN, *Halieut:* lib. i. V. 554.

† Adfertur squillas inter muræna natantes,
In patinâ porrecta: "hæc gravida," inquit,
"Capta est."—HOR. lib. ii. Sat. 8. lin. 46.

say and Dr. Dutrochet. Mr. Lindsay's observations are to be met with in a MS. preserved in the library of the Royal Society, which is dated July 1790: this essay is alluded to by Dr. Smith in his "Introduction to Botany." Dr. Dutrochet's experiments were published in his "Recherches anatomiques et physiologiques sur la Structure intime des Animaux et des Végétaux," which appeared in 1824. With the latter author the reputation of originality is likely to rest: not undeservedly, indeed, as there is no reason to suppose that *his* experiments were suggested by a knowledge of those performed by Lindsay. It is, however, an act of literary justice to secure to Mr. Lindsay the credit of undoubted priority in describing the phenomena which he noticed in common with Dutrochet. I have drawn up the following remarks partly for this purpose—partly to have an opportunity of mentioning some circumstances which escaped the observation of both experimentalists.

The leaves of the Mimosa Pudica consist either of one or two or three pairs of leaflets, and occasionally terminate by an odd one. Each leaflet bears from twenty to sixty subleaflets, which are disposed in pairs. The petiole or stalk of each leaf, at the extremity which is attached to the branch or stem of the peant, swells into an intumescence varying from three to five in length. A similar intumescence, of proportionate dimensions, is seen upon each subpetiole, where it is articulated with the petiole, and upon the base of the stalk of each subleaflet: the intumescence is the part in which motion takes place.

During the day-time the petioles are observed to have a direction upwards, or rather to form an acute angle with the upper part of the stem or branch, to which they are attached: the subpetioles are divergent: the subleaflets are spread out, so as to lie nearly in one plane. (*Fig.* 1.)

During the night the petioles are found to be depressed; the subpetioles to be drawn together, the subleaflets folded, the upper or solar surfaces of each pair being brought into contact. (*Fig.* 2.)

The leaves rise, the leaflets diverge, and open by throwing down their subleaflets, at daybreak: the opposite changes occur about sunset. The experiments that are to be described, are supposed to be performed in the day-time.

Fig. 1.

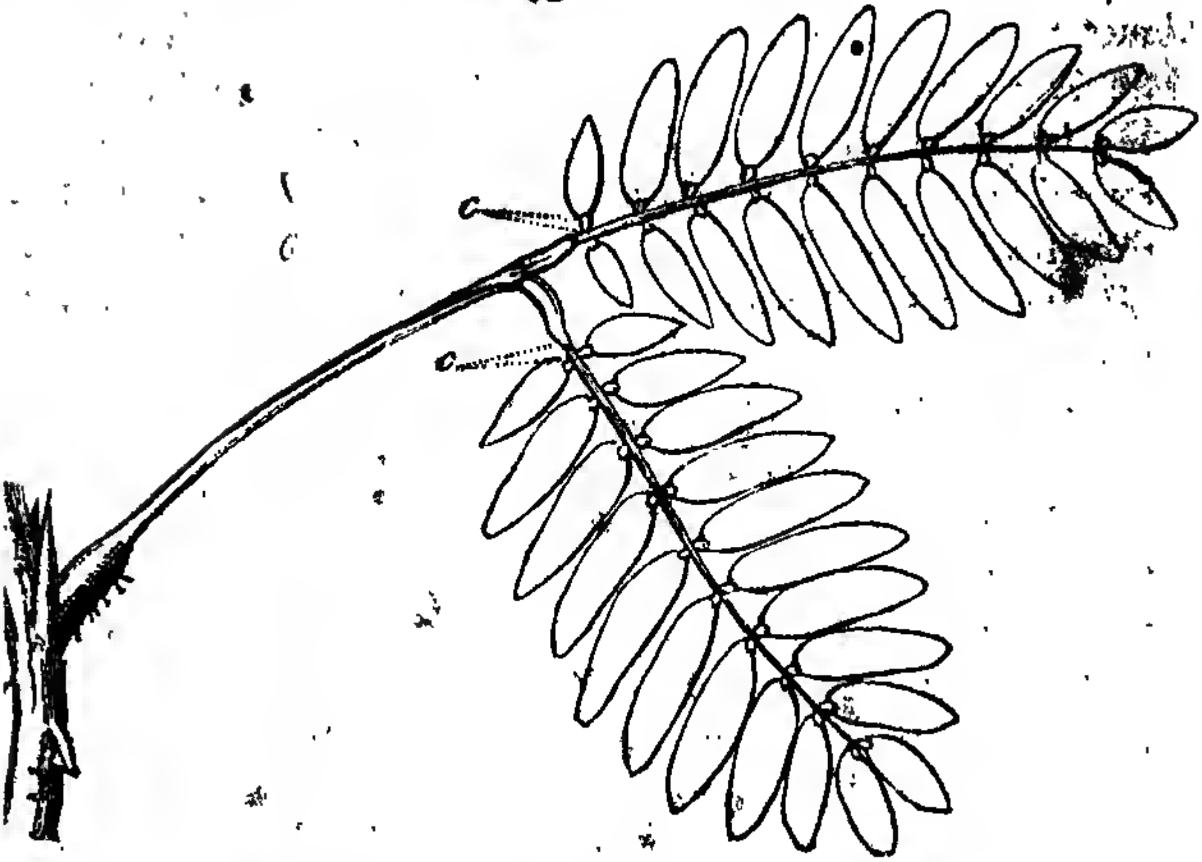
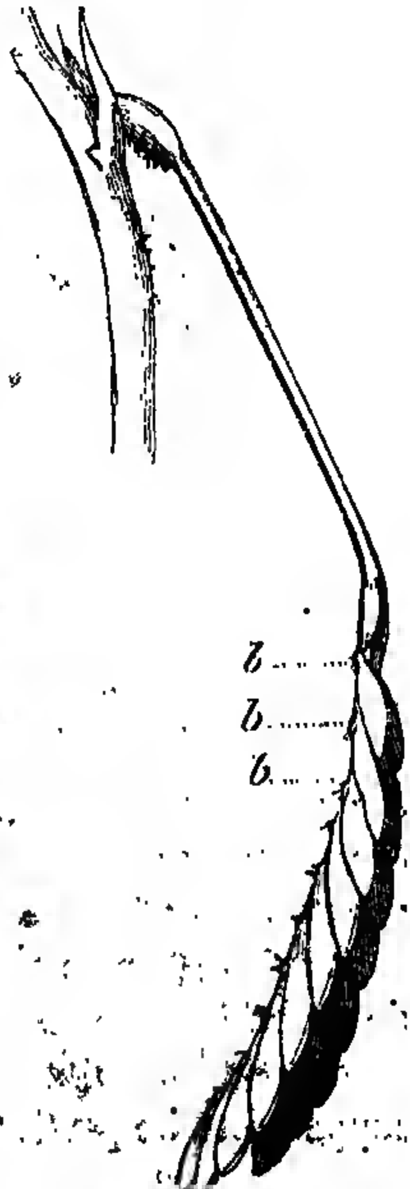


Fig. 2.



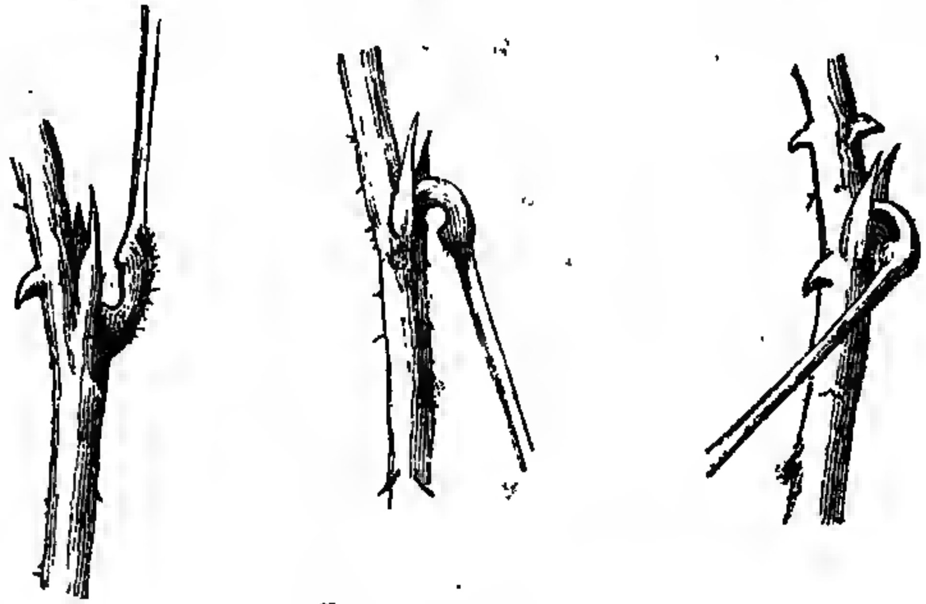
If a terminal subleaflet be pinched with forceps, or cut with scissors, it rises, together with its fellow; then the next pair rise; then the next; and so on in succession, till all the pairs of subleaflets upon the same subpetiole are folded. In a little time afterwards, the petiole is bent downwards at its intumescence; and in a few seconds more the remaining leaflets upon the same petiole fold their subleaflets in pairs, from the base towards the point of the leaflet.

If a subleaflet be burnt, instead of being cut or pinched, the phenomena above described occur more rapidly: and after they have taken place, the adjoining leaves upon the same branch are bent down in succession, their leaflets brought together, and their subleaflets folded. If the plant be very vigorous and lively, an impression

made upon one leaf affects the rest in succession. It is well known that the stem, branches, flowers, and roots of the sensitive plant have no motion. But M. Desfontaines observed that, on touching the roots with sulphuric acid, the leaves become folded; and M. Dutrochet obtained a similar result on burning either the flower or the stem.

If the plant be shaken, all the leaves are simultaneously thrown down, and their leaflets folded. Mr. Lindsay attempted to elucidate the action of the intumescence in raising and depressing the petiole, in the following manner. He cut out a portion from the upper or solar surface of the intumescence; after which he found that the petiole, upon recovering, rose higher than before, (*Fig.* 3.) From another leaf he removed the inferior portion of the intumescence: he found, upon *this* injury, that the leaf declined more than before, and did not again rise, (*Fig.* 4.) He noticed that a thin slice, pared from

Fig. 3. *Fig.* 4. *Fig.* 5.



either surface of the intumescence, has a like effect, but in a less degree than a deep excision: and he found that when similar experiments are made upon the intumescence of the subpetiole, there is no essential difference in the result.

Thus Mr. Lindsay discovered, that the force which raises the petiole exists in the lower part of the intumescence, and that which depresses it, in the upper. He seems to have considered that the temporary excess of force in either part is produced by an impulsion of the sap from the vessels of the yielding portion into those of the opposite portion.

Dr. Dutrochet viewed these phenomena in some respects more justly. He remarked, in addition to what Lindsay had observed, that if, instead of the upper and under surface, the lateral part of the intumescence be removed, the petiole becomes not raised or deflected, but inclined towards the side on which it is injured (*Fig*. 5); and that if longitudinal slices of the upper, or under, or lateral portions of the intumescence are immersed in water, these separate slices immediately become incurvated, that edge being concave which looks towards the axis of the intumescence. From these facts Dutrochet inferred that the texture of the intumescence possesses some modification of irritability; that, when excited, each length of the intumescence (to use a very imperfect expression) forcibly assumes an incurvated figure, like a curved spring returning from a state of temporary extension; that the petiole is raised, when the action of the lower part of the intumescence predominates; is depressed, when the upper portion acts with increased energy.

Mr. Burnett and myself had arrived at very similar conclusions respecting the agency of the intumescence, before we became acquainted with the inquiries of Lindsay and Dutrochet.

In Dutrochet's able researches, a more exact analysis, however, was obtained of the functions of this part. He discovered that the cortex of the intumescence is the seat of its irritability: for upon wholly removing the bark, so as to expose the ligneous substance, the petiole was found to have been rendered motionless. Nevertheless, the intumescence, thus mutilated, remains capable of transmitting an impression made upon its leaflets to the leaves adjoining. Dutrochet further ascertained, that the ligneous substance alone is fitted to convey the peculiar stimulus, which spreads, from a point of the plant that has been irritated, to the adjoining leaves.

The experiments already mentioned appear to explain the mode in which the elevation and depression of the petiole, and the divergence and approximation of the subpetioles are produced. It is probable that the contrivance for folding and expanding the subleaflets is of a similar nature. Mr. Burnett and myself conjectured that each subleaflet is raised by the under part of the intumescence that exists at its base, and de-

pressed by some action of the upper portion of the same intumescence. In trying the soundness of this hypothesis, we met with the following evidence in its favour:—

Mr. Lindsay had observed, that at the moment when the petiole is depressed, the under part of its intumescence assumes a deeper colour. But the under part of the intumescence of the petiole is the portion which is shortened during its depression, and which is overcome on this occasion by the superior force of the upper portion.

Now it is to be remarked that in the subleaflets the upper part of the little intumescence belonging to each corresponds, in one respect alluded to, with the lower portion of the intumescence of the petiole; *it is the portion shortened when the leaf is folded.* And we found, upon examination, that it likewise distinctly changes colour at the moment when the subleaflet rises, while the under surface of the intumescence of the subleaflet does not change its hue.

In pursuing this inquiry, another point of correspondence between the mechanism which depresses the petiole, and that which raises the subleaflets, was stated, which has yet additional interest.

When the plant is not in its most lively state, the under surface of the intumescence of the subleaflet (*b*, *Fig.* 2,) and the upper surface of the intumescence of the petiole (*a*, *Fig.* 6,)

Fig. 6.

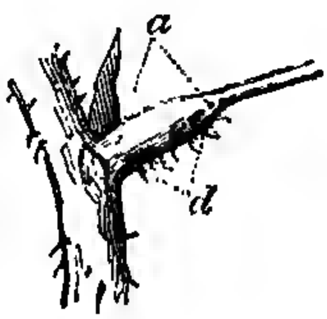


Fig. 7.

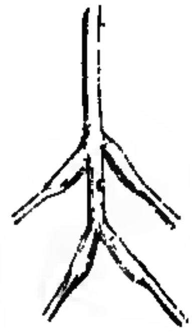
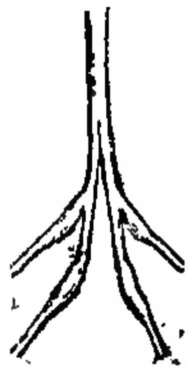


Fig. 8.



may be pricked with a needle, without producing action. But if the opposite surfaces, those namely, which change colour and are shortened when the petiole is depressed and the subleaflets folded, are touched with the point of the needle these actions are instantaneously produced. Here the sub-

leaflet is most delicately sensible; *a slight touch* with the point of a needle upon the upper surface of the intumescence of the subleaflet (*c*, *Fig.* 1,) causes the single subleaflet so stimulated to rise; and in this manner all the subleaflets upon one side of a leaflet may be raised, their fellows remaining expanded. if the touch be something sharper, the fellow subleaflet rises at the same time; if ruder still, the next pair of leaflets fold directly afterwards, and the irritation then proceeds entirely through the leaflet. But the most satisfactory and curious results are obtained on stimulating the extension surface of the intumescence of the petiole. The needle may be applied to every point upon the upper or solar half of the intumescence of the petiole (*a*, *Fig.* 6,) without producing any visible effect; but if the irritation be applied upon the under half, (*d*, *Fig.* 6,) either quite below or laterally, the petiole is immediately depressed. The transition is abrupt from the surface against which the needle may be made to prick, without exciting action, to one which, when the needle reaches it, causes the petiole to be instantaneously thrown down.

It appears, therefore, that each intumescence has a surface especially adapted to receive mechanical impressions; which surface is placed on the side of the intumescence opposite to that, by which the consequent motion is produced. A curious but vague analogy may be traced between these surfaces of the sensitive plant and the organs of sense in animals.

We painted with a thick layer of lamp-black in oil the intumescence of different petioles in different ways; the upper surface of one, the under surface of another, the side of a third. The experiment was followed by no sensible effect. After a few minutes the petioles, which had been thrown down by the operation, rose again in each case, and fell again as readily as before upon being stimulated afresh.

We tried what result would ensue upon slitting the intumescence of the petiole horizontally. The petiole, after this injury, did not recover its usual direction; the intumescence appeared to have wholly lost its properties; the leaf seemed to depress the petiole by its weight alone, yet the leaflets expanded, and exhibited their usual irritability, upon the depending stalk. The same effect, however, was observed, when the

intumescence was divided by a longitudinal incision, made vertically instead of horizontally.

I have already mentioned that Dutrochet discovered that the ligneous fibre is the channel, along which an impression is conveyed from one part to another. Mr. Burnett and myself had made one or two experiments upon the course which the irritation follows when spreading from leaflet to leaflet, where several are placed upon the same petiole.

If the upper third of a petiole bearing four leaflets be divided longitudinally, the irritability of the leaflets remains for many days unimpaired; upon cutting with scissors one subleaflet after the plant has recovered itself, the irritation is observed to descend the wounded leaflet, and then to pass to that adjoining upon the same side of the petiole: afterwards the petiole falls, but there the effect stops; it does not extend to the two other leaflets; the direct route is cut through, and the irritation seems to find no circuitous way, as might have been expected, perhaps through the intumescence of the petiole back again to the leaflets, on its summit. If on a petiole, bearing four leaflets, a lateral incision be made, cutting the petiole half through it at a point between the two leaflets which are situated on one side, upon irritating either of the leaflets, between which the incision has been made, it folds its subleaflets; then the two opposite leaflets fold *their* subleaflets; and *last of all*, the leaflet next adjoining that first irritated, but isolated from it by the incision, becomes folded.

In the few remarks which I have thus put together, I have quoted Lindsay and Dutrochet only as far as their researches anticipated my own: I leave unnoticed many experiments, in several of which these authors are again found to have accidentally coincided. The experiments to which I allude do not, however, serve to illustrate the nature of the motion exhibited by the sensitive plant, to the examination of which subject alone my attention was, in the present instance, directed, in the expectation that it might throw light upon the obscure and interesting subject of muscular action.

I remain, my dear Sir, Your's truly,

HERBERT MAYO.

19, *George Street, Hanover Square,*
August 29, 1827.

Experiments on the Nature of Labarraque's disinfecting Soda Liquid. By M. Faraday, F.R.S., Corr. Mem. R. Acad. Sciences, Paris, &c. &c.

1. The following experimental investigations relate to the nature of that medicinal preparation which M. Labarraque has lately introduced to the world, and named *Chloride of oxide of Sodium.* They were occasioned by the accounts which were given of this and other substances of similar power, to the members of the Royal Institution, at two of their Friday evening meetings*; the value of the preparation, the uncertainty of its nature, and the inaccuracy of its name, all urging the inquiry.

2. In the first instance the inquiry was directed to the nature of the action exerted by chlorine gas upon a solution of carbonate of soda, questions having arisen in the minds of many, whether it was or was not identical with the action exerted by the same gas on a solution of the caustic alkali, and whether carbonic acid was evolved during the operation or not. Chlorine gas was therefore carefully prepared, and after being washed was sent into a solution of carbonate of soda, in the proportions directed by M. Labarraque; *i. e.* 2800 grains of crystallized carbonate of soda were dissolved in 1.28 pints of water; and being put into a Woulfe's apparatus, two-thirds of the chlorine evolved from a mixture of 967 grains of salt with 750 grains of oxide of manganese, when acted upon by 967 grains of oil of vitriol, previously diluted with 750 grains of water, were passed into it; the remaining third being partly dissolved in the washing water, and partly retained in the open space of the retort and washing vessel. The operation was conducted slowly, that as little muriatic acid as possible might be carried over into the alkali. The common air ejected from the bottle containing the solution was collected and examined; but from the beginning to the end of the operation not a particle of carbonic acid was disengaged from the solution, although the chlorine was readily absorbed. Ultimately a liquid of a very pale

* See the last volume of this Journal, pp. 211, 460.

yellow colour was obtained, being the same as M. Labarraque's soda liquor, and with which the investigations were made that will hereafter be described.

3. An experiment was then instituted, in which the effect of excess of chlorine, upon a solution of carbonate of soda of the same strength as the former, was rendered evident. The solution was put into two Woulfe's bottles, the chlorine well washed and passed through, until ultimately it bubbled through both portions without absorption of any appreciable quantity. As soon as the common air was expelled, the absorption of the chlorine was so complete in the first bottle, that no air or gas of any kind passed into the second, a proof that carbonic acid was not liberated in that stage of the experiment. Continuing the introduction of the chlorine, the solution in the first bottle gradually became yellow, the gas not being yet visible by its colour in the atmosphere above the solution, although chlorine could be detected there by litmus paper. Up to this time no carbonic acid gas had been evolved; but the first alkaline solution soon acquired a brighter colour, and now carbonic acid gas began to separate from all parts of it, and passing over into the second bottle, carried a little chlorine with it. The soda solution in the first bottle still continued to absorb chlorine, whilst the evolution of carbonic acid increased, and the colour became heightened. After some time the evolution of carbonic acid diminished, smaller quantities of the chlorine were absorbed by the solution, and the rest passing into the atmosphere in the bottle, went from thence into the second vessel, and there caused the same series of changes and actions that had occurred in the first. The solution in the first bottle was now of a bright chlorine yellow colour, and the gas bubbled up through it as it would through saturated water.

4. When the chlorine had saturated the soda solution in the second bottle, and an excess of gas sufficient to fill several large jars had been passed through the whole apparatus, the latter was dismounted, the solutions put into bottles and distinguished as the saturated solutions of carbonated soda; they were of a bright greenish-yellow colour, and had an insupportable odour of chlorine.

5. The saturated solution (4) was then examined as to the

change which had been occasioned by the action of the chlorine. It bleached powerfully, and apparently contained no carbonated alkali: but when a glass rod was dipped into it and dried in a warm current of air, the saline matter left, when applied to moistened turmeric paper, reddened it considerably at first, and then bleached it; and this piece of paper being dried and afterwards moistened upon the bleached part, gave indications of alkali to fresh turmeric paper.

6. A portion of the saturated solution (4) being warmed, instantly evolved chlorine gas, then assumed a dingy appearance, and ultimately became nearly colourless; after which it had an astringent and saline taste. Being evaporated to dryness at a very moderate temperature, it left a saline mass, consisting of much common salt, a considerable quantity of chlorate of soda, and a trace of carbonate of soda. This mixture had no bleaching powers. The dingy appearance, assumed in the first instance, was found to be occasioned by a little manganese which had passed over into the solutions, notwithstanding the care taken in evolving and washing the gas.

7. From these experiments it was evident that when chlorine was passed *in excess* into a solution of carbonate of soda (3), the carbonic acid was expelled, and the soda acted upon as if it were caustic, a mixture of chloride of sodium and chlorate of soda being produced: with the exception of the small portion of carbonate of soda which, it appears, may remain for some time in the solution in contact with the excess of chlorine at common temperatures, without undergoing this change. The quantities of chloride of sodium and chlorate of soda were not ascertained, no doubt being entertained that they were in the well-known proportions which occur when caustic soda is used.

8. The Labarraque's soda liquor which had been prepared as described (2), was now examined relative to the part the chlorine played in it, or the change the alkali had undergone, and was soon found to be very different to that which has been described, as indeed the experiments I had seen made by Mr. Phillips* led me to expect. The solution had but little odour of chlorine, its taste was at first sharp, saline, scarcely at

* See Vol. I. of this Journal, p. 461; and *Phil. Mag.* N. S., I. 376.

all alkaline, but with a persisting, astringent biting effect upon the tongue. When applied to turmeric paper, it first reddened and then bleached it.

9. A portion of the solution (2) being boiled, gave out no chlorine; it seemed but little changed by the operation, having the same peculiar taste, and nearly the same bleaching power as before. This is a sufficient proof that the chlorine, though in a state ready to bleach or disinfect, must not be considered as in the ordinary state of solution, either in water or a saline fluid; for ebullition will freely carry off the chlorine under the latter circumstances.

10. A portion evaporated on the sandbath rather hastily, gave a dry saline mass, quite unlike that left by the *saturated solution* already described (6); and which, when dissolved, had the same astringent taste as before, and bleached solution of indigo very powerfully: when compared with an equal portion of the unevaporated solution, which had been placed in the mean time in the dark, its bleaching power upon diluted sulphate of indigo was 30, that of the former being 76. Another portion, evaporated in a still more careful manner, gave a mass of damp crystals, which, when dissolved, had the taste, smell, and bleaching power of the original solution, with almost equal strength.

11. These experiments shewed sufficiently that the whole of the chlorine had not acted upon the carbonate of soda to produce chloride of sodium, and chlorate of soda; that much was in a peculiar state of solution or union which enabled it to withstand ebullition, and yet to act freely as a bleaching or disinfecting agent; and that probably little or none had combined with the sodium, or been converted into chloric acid. To put these ideas to the test, two equal portions of the Labarraque solution were taken; one was put into a large tube, closed at one extremity, diluted sulphuric acid was added till in excess, and then air blown through the mixture by a long small open tube, proceeding from the mouth, for the purpose of carrying off the chlorine; the contents of the tube were then heated nearly to the boiling point, air being continually passed through. In this way all the chlorine which had combined with the carbonated alkali without decomposing it, was set free by the sulphuric acid, and carried off by the current of air and vapour, whilst any which had acted chemically upon the alkali would,

after the action of the sulphuric acid, be contained in solution as muriatic and chloric acids, and from the diluted state of the whole, would not be removed by the after-process, but remain to be rendered evident by tests. The other portion being diluted, had sulphuric acid added also in excess, but no attempt was made to remove the chlorine. Equal quantities of these two portions in the same state of dilution were then examined by nitrate of silver for the quantities of chlorine sensible in them, and it was found that the latter portion, or that which retained the whole of the chlorine thrown into it, contained above sixty times as much as the former.

12. Now although it may be supposed that in the former portion that part of the chlorine, which, in acting energetically, had produced chloric acid, could not be detected by the nitrate of silver, yet more than a sixth of the small portion which remains cannot be thus hidden; and even that quantity is diminished by the sulphuric acid present in excess, which tends to make the chlorine in the chlorate sensible to nitrate of silver: so that the experiment shews that nearly 59 parts out of 60 of the chlorine in M. Labarraque's liquid are in a state of weak combination with the carbonated alkali, and may be separated by acids in its original condition; that this quantity is probably wholly available in the liquid when used as a bleaching or disinfecting agent; that little, if any, of the chlorine forms chloride of sodium and chlorate of soda with the alkali of the solution; and that the portion of chlorine used in preparing the substance which is brought into an inactive state, is almost insensible in quantity.

13. The peculiar nature of this compound or solution, with the results Mr. Phillips had shewn me (8), obtained by evaporation of a similar preparation to dryness, induced me to try the effects of slow evaporation, crystallization, heat, and air upon it. In the first place five equal portions of the solution prepared by myself were measured out: two were put into stoppered bottles, two were put into basins and covered over with bibulous paper, and one was put into a basin which was left open; all were set aside in an obscure place, and remained from July 16th to August 28th. Being then examined, the portions in the basins were found crystallized and dry; the crystals were large and flat, striated and imperfect, resembling those formed

in a similar way from carbonate of soda. They were not small and acicular, were nearly alike in the three basins, and had effloresced only on a few minute points. A part of one portion, when dissolved, gave a solution, having an alkaline taste, without any of the pungency of Labarraque's liquid; and which, when tested by turmeric paper, reddened, but did not bleach it.

14. One of these portions that had effloresced least was selected, and being dissolved, was compared in bleaching power upon diluted sulphate of indigo, with one of the portions of solution that had been preserved in bottles. The former had scarcely any visible effect, though sulphuric acid was added to assist the action; a single measure of the indigo liquor coloured the solution permanently blue, whereas seventy-seven such measures were bleached by the portion from the bottle. Hence the process of slow crystallization had either almost entirely expelled the chlorine, or else had caused it to react upon the alkali, and by entering into strong chemical combination as chloride and chlorate, had rendered it inert as a bleaching or disinfecting agent.

15. From the appearance of the crystals there was no reason to expect the latter effect, but to put the question to the proof, one of the evaporated portions, and one of the fluid portions contained in the bottles, were acted upon by sulphuric acid, heat, and a current of air, in the manner already described (11), to separate the chlorine that had not combined as chloride or chlorate. They were then compared with an equal portion of the solution, which retained all its chlorine, nitrate of silver being used as before: the quantity of chloride indicated for the latter portion was 60 parts; whilst that of the fluid portion deprived of as much free chlorine as could be, by sulphuric acid and blowing, was 6 parts; and for the evaporated and crystallized portion, similarly cleared of free chlorine, only 1.5 parts.

16. This result, as compared with the former experiment of a similar kind (11), shewed, that though reaction of the chlorine on the carbonate had taken place in the evaporated portion, it was only to a very slight extent, since the chlorine was almost as much separated from it by the process altogether, as it had been from the recent preparation by sulphuric acid, blowing, and heat. The experiment shewed also that there

was a gradual reaction of the chlorine and alkali in the fluid preparation, proceeding to a greater extent than in the evaporated portion; for chlorine, equal to five parts, was found by the nitrate of silver to remain. Hence this preparation is one which deteriorates even in the small space of forty-three days. Whether the effect will proceed to any great extent, prolonged experiments only can shew.

17. From an experiment made upon larger quantities of the Labarraque liquor, it would appear that the force of crystallization alone is sufficient to exclude the chlorine. A quantity was put into an evaporating basin, and left covered over with paper from July 16th to August 28th. Being then examined, a few large crystals were found covered over with a dense solution; the whole had the innocuous odour of Labarraque's fluid, and the fluid the usual acrid, biting taste. The crystals being separated, one of the largest and most perfect was chosen, and being well wiped on the exterior, and pressed between folds of bibulous paper, was rubbed down in water, so as to make a saturated solution. This had no astringent taste like that of Labarraque's fluid, or the mother-liquor, but one purely alkaline; and when applied to turmeric paper, reddened, but did not bleach it. Equal portions of this saturated solution and of the mother-liquor were then compared in bleaching power, acid being added to the former to assist the effect: it was found, notwithstanding that portions of mother-liquor must have adhered to the crystal, that its solution had not $\frac{1}{21}$th part the power of the mother-liquor. This, in conjunction with the other experiments, is a striking instance of the manner in which the carbonate of soda acts, as a simple substance, with the chlorine in the solution. The crystal itself had never been in contact with the air: but whether it should be considered as the excess of carbonate of soda only which crystallized; or whether it is essential to the formation of these crystals that chlorine should simultaneously be given off into the air; or what would take place, if the water were abstracted without the evolution of chlorine, I have not determined.

18. Notwithstanding the perfect manner in which the chlorine may be thus separated by crystallization and slow evaporation to dryness, yet it is certain that by quick evaporation a

substance apparently quite dry, may be obtained, which yet possesses strong bleaching power. In one experiment, where, of two equal portions, one had been evaporated in the course of twenty-four hours to dryness upon the warm part of a sand-bath, when compared with the former, it had not lost more than one-third of its bleaching power.

19. With the desire of knowing what effect carbonic acid would have on Labarraque's fluid, and whether it possessed in a greater or smaller degree the power of ordinary acids to expel the chlorine, portions of the solution were put into two Woulfe's bottles, and a current of carbonic acid gas passed through them. The gas was obtained from sulphuric acid and whitening in a soda-water apparatus, and was well washed in water. The stream of gas brought away small portions of chlorine with it, but they were not sensible to the smell, and could only be detected by putting litmus paper into the current. An immense quantity of gas, equal to nearly 1300 times the volume of the fluid, was sent through; but yet very little chlorine was removed, and the bleaching powers of the fluid were but little diminished, though it no longer appeared alkaline to turmeric paper. Air was then passed through the solution in large quantity; it also removed chlorine, but apparently not quite so much as carbonic acid.

20. One other experiment was made upon the degree in which the carbonate of soda in Labarraque's liquor resisted decomposition by the chlorine, even at high temperature. Two equal portions of the fluid were taken, and one of them boiled rapidly for fifteen minutes; both were then acted upon by sulphuric acid, blowing, and heat, as described (11), and the two were then tested by nitrate of silver, to ascertain the quantity of chlorine remaining: it was nearly three times as much in the boiled as in the unboiled portion; and by comparing this with the results before obtained (11), it will be seen that, after boiling for a quarter of an hour, not more than a twentieth part of the chlorine had acted upon the alkali, to form chloride and chlorate.

21. It would seem as if I were unacquainted with Dr. Granville's paper upon this subject, published in the last volume of this Journal, p. 371, were I to close my remarks without taking

any notice of it. Unfortunately, Dr. Granville has mistaken M. Labarraque's direction, and by passing chlorine, to "complete saturation," through the carbonate, instead of using the quantities directed, has failed in obtaining Labarraque's really curious and very important liquid; to which, in consequence, not one of his observations or experiments applies, although the latter are quite correct in themselves.

Royal Institution, Sept. 3, 1827.

HIEROGLYPHICAL FRAGMENTS; *with some Remarks on* ENGLISH GRAMMAR. *In a Letter to the Baron William Von* HUMBOLDT. By a Correspondent.

My dear Sir,

I AM happy to tell you that our prospects of new documents from Egypt are very rapidly increasing: Mr. BURTON has had the good fortune to discover at length, in a mosque, the triple inscription for which he has been some years in search; and he has been negociating with the Pacha for its removal. From its magnitude and state of preservation, there is every reason to believe that it will rival the pillar of Rosetta in its importance, and I sincerely hope that it will tend to check the wildness of conjecture, which has been rioting without bounds in the regions of Egyptian literature. Mr. Tattam is printing a Coptic grammar, and I am preparing an Appendix, which is to contain the rudiments of an Enchorial Lexicon: I ardently wish that Mr. Burton's inscriptions may come to my assistance before I complete it. I have received nothing from France or from Germany for these four years past: even what is published seems by some fatality to have been withheld from me; and the booksellers send no answers to my commissions. I trust your brother will not forget his kind promise to think of me at Berlin.

I have to thank him and you for your obliging present of your *Letter to Abel Remusat on the Genius of the Chinese Language*, which has greatly interested me: the best return that I can make will be to give you some remarks which have occurred to me on the language of hieroglyphics in general,

and on the character of the English language, which seems to approach, in its simplicity, as you have yourself observed, to the natural structure of the oldest languages, immediately related to the hieroglyphical form of representation. I fear, however, that I must apologize to you for the want of method with which I shall be obliged at present to throw my fragments together: but it may be allowable to make some difference between a letter and a finished essay.

Hieroglyphics, in their primitive form, are scarcely to be considered in any case as simply a mode of expressing an oral language: they may be a direct and independent representation of our thoughts, that is, of recollections, or sentiments, or intentions, collateral to the representation of the same thoughts by the language of sounds. We find, in many of the Egyptian monuments, a double expression of the same sense: first, a simple picture, for instance, of a votary presenting a vase to a sitting deity; each characterized by some peculiarity of form, and each distinguished also by a name written over him; and this may be called a pure hieroglyphical representation, though it scarcely amounts to a language, any more than the look of love is a language of a lover. But we universally find that the tablet is accompanied by a greater variety of characters which certainly do constitute a language, although we know little or nothing of the sounds of that language; but its import is, that "such a king offers a vase to the deity;" and on the other side, that "the deity grants to the king health and strength, and beauty and riches, and dominion and power." It is common to see, in these inscriptions, a number of characters introduced, which are evidently identical with some of those in the tablets: and however some of them may occasionally have been employed phonetically, there can be no question of the nature of the changes which their employment must have gone through before they assumed the character of sounds: but this is altogether a separate consideration, and foreign to the present purpose.

Now it is obvious that objects, delineated with the intention of representing the originals to the eye by their form, must necessarily be nouns substantive; and that the picture, containing no verb whatever, can scarcely be said to constitute

either a positive or a negative assertion. At the same time, it must be allowed that a picture of King George the Fourth's coronation, with the date 19 July 1821, could scarcely be considered otherwise than as asserting a historical truth; and if any emblem of Truth were attached to it, or if it were deposited among the records of other historical facts, it would be equivalent to the expression, "George IV. crowned in July 1821," which *scarcely* wants the verb *was* to convert it into a positive assertion of a fact.

Strictly speaking, however, there seems to be no direct mode of supplying the want of the verb *is* or *was* in pure hieroglyphical writing; and if any such sign was employed in the Egyptian or the old Chinese hieroglyphics, its introduction must have been arbitrary or conventional; like the employment of a postulate in mathematics. Every other part of a language appears capable of being reduced, with more or less circumlocution, to the form of a noun substantive; and the English language appears to approach to the Chinese in the facility with which all the forms of grammar may be shaken off.

There is, however, often occasion, in such cases, for a certain degree of metaphor approaching to poetical latitude; and hence it may happen that the least literary nations are sometimes the most poetical. It is, in fact, impossible to exclude metaphor altogether from the most prosaic language; and it is frequently difficult to say where metaphor ends and strict logical prose begins; but by degrees the metaphor drops, and the simple figurative sense is retained. Thus we may say *liquid ruby* with the same exact meaning as *crimson wine;* and yet *ruby* would never be called an adjective, though employed merely to express the colour: in *coral lips*, however, the *coral*, first used metaphorically, is converted by habit into an adjective, and the expression is considered as synonymous with *labri corallini.*

The general custom in English is to place the figurative substantive, used as an adjective by comparison, or by abstraction, before the name which retains its proper sense: thus a chestnut horse is a chestnut like or chestnut coloured horse; a horse chestnut is a coarse kind of chestnut: and in this manner we are enabled to use almost every English noun substantive as an adjective, by an ellipsis of the word *like*, which,

if inserted entire or abridged, would make a real adjective of the word, as war*like*, friend*ly*. But this omission of the termination, like other figures of speech, is easily forgotten in the ordinary forms of language; and the Germans, as well as the English, make use of almost all their substantives in the place of adjectives, though they are more in the habit of continuing them into single long words. When, however, the substantives are so used, they generally become by abstraction real adjectives: for we seldom think of a *chestnut*, in speaking of the colour of a horse; but the idea of a light brown coat, with an ugly pale-red mane and tail, and a fidgety temper, is very likely to occur to us: and in a horse chestnut the idea of a horse is out of the question; we only think of a coarse fruit which a man cannot eat: so that the true sense, in both these instances, is that of a quality; but *coral lips* and *ivory hands* are rather elliptical expressions, composed of two substantives, which might fairly be represented hieroglyphically by the assistance of a branch of coral and an elephant's tusk. But to describe an abstract quality by any hieroglyphic character, representative of form only, would be generally impossible: colours might be imitated, if we supposed coloured figures to be employed; but other simple ideas, such as those of sound or touch, could never be immediately presented to the eye; and some circuitous invention would always be required for their representation.

Horne Tooke has shewn, with considerable felicity of illustration, that all the parts of speech may be resolved into the noun and the verb; but he has not pointed out so clearly that every verb may be resolved into a noun and the single primitive verb is or was, which, in this sense, may be said to be the only essential verb in any language; as we find, indeed, in the Coptic, that almost every noun becomes a verb, either by the addition of PE, or sometimes even without it. Thus, *the morning* BLUSHES is synonymous with *the morning* IS *red; he loves justice*, with *he* IS *a lover of justice;* and *I* AM *an Englishman*, with *the person now speaking* IS *an Englishman*. But this must be understood of is, was, or will be, in all its tenses; the idea of time, if expressed, being an essential part of the verbal sense.

I confess that some of these reflections have occurred to me in looking over a very singular work, which I had the curiosity

to take up, in order to see what kind of information could be possessed by a person notoriously and professedly ignorant of the origin and relations of the language which he attempts to teach; and, in short, what kind of light could be diffused by an apostle of darkness. Blunders, and some of them ridiculous enough, must, of course, be found in the works of such a person, but most of them are such as every schoolboy might correct; and there really is so much of sagacity in some of Mr. Cobbett's remarks on the errors of others, that they well deserve the attention of such as are ambitious to write or speak with perfect accuracy.

I shall not attempt to enter into a regular criticism of this *Grammar;* I shall merely make a few miscellaneous observations, as they have occurred to me in reading it, several of which would be equally applicable to the best of the existing works of a similar nature.

In Letter III we are told that *long* and *short,* though adjectives, do not express *qualities,* but merely dimension or duration; from a singular misconception of the proper sense of the word *quality.* We find, in Letter IV, the rule given by most grammarians, though not by all, that the article A becomes AN, when it is followed by any word beginning with a vowel; but it is surely more natural to follow the sound than the spelling, and, as we should never think of saying an *youthful* bride, it seems equally incorrect to say an *useful* piece of furniture; for the initial sound is precisely the same. In the same manner A *unit* and A *European,* seems to sound more agreeable than AN; and the best speakers appear to adopt this custom.

Letter VIII gives us a rule for doubling the last letter of a verb in the participle if an accent is on the last syllable: but it should be observed that the L is doubled, whether accented or not, as in *caballing, travelled, levelled, cavilled, controlled.* The same letter contains a "List of verbs, which, by some persons, are erroneously deemed irregular," and which have been so deemed from the time of our German and Saxon ancestors, though Mr. Cobbett thinks it would be more philosophical to conjugate them regularly. Thus we may see at once that *freeze* may as well give us *frozen,* as *frieren* gives the Germans *gefroren;* that *hang* may make *hung* or *hanged,* accord-

ing to its sense, as in German we have *hienge* from *hängen*, and *hängte* from *hängen*, to execute. For *sling* and *slung*, we have authority in *schlingen*, *geschlungen*, for *spring* and *sprung* in *springen* and *gesprungen*; for *swollen*, *swam* or *swum*, and *swung*, in *geschwollen*, *geschwommen*, and *geschwungen*. And it is quite clear from these examples that "the bad practice of abbreviating, or shortening," has nothing to do with the matter.

In Letter XIV we have a very distinct examination of a rule in punctuation which has been commonly adopted by good printers, without so distinct a description of its foundation. "Commas are made use of, when phrases, that is to say 'portions' of words, are 'throw*ed*' into a sentence, and which are not absolutely necessary to assist in its grammatical construction." In a word, two commas are very nearly equivalent to the old fashioned parenthesis. Again, "the apostrophe ought to be called the mark not of elision, but of *laziness* and *vulgarity*;" a remark made in truly classical taste, which might have been extended with perfect propriety to the subject of the next paraparaph, the *Hyphen*, the insertion of which is, to make it uncertain whether the words united by it are one word or two. He goes on admirably in the next page. "*Notes*, like parentheses, are *interrupters*, and much more troublesome interrupters, because they generally tell a much longer story. The employing of them arises, in almost all cases, from confusion in the mind of the writer. He finds the matter *too much for him*. He has not the talent to work it all up into one lucid whole; and, therefore, he puts part of it into *Notes*" . . . . . "Instead of the word *and*, you often see people put *&*. For what reason I should like to know. But to this *&* is sometimes added a *c*; thus, *&c*. *And* is, in Latin, *et*, and *c* is the first letter of the Latin word *caetera*, which means the like, or *so on*. This abbreviation of a foreign word is a most convenient thing for such writers as have too much indolence or too little sense to say fully and clearly what they ought to say. If you *mean* to say *and the like*, or, *and so on*, why not say it ? . . . The abbreviation is very frequently made use of *without the writer having* any idea of its import." But it is surely a mischievous maxim, never to "think of *mending* what you write.' Let it *go*. No

patching; no *after painting.*" On the other hand he is right in protesting "against the use of what, by some, is called the *dash*. Who is to know what is intended by the use of these *dashes?*.... It is a cover for ignorance as to the use of points; and it can answer no other purpose."

In Letter XV, there is a singular conceit with regard to the keeping up a distinction between *a* and *an*, where it is insisted that we must not say "*a* dog, cat, owl, and sparrow;" because owl requires *an;* "and that it should be, a dog, a cat, an owl, and a sparrow;" which is certainly better, and would be so, even if there were no owl in the question.

Letter XVII. The criticism on Milton's "than *whom* none higher sat," is perfectly correct. *Than* is never a preposition, and is simply a variation from the older *then*, both in English and in German. *John is better than James* means simply John is good first, then James: *er* is *eher* or *e'er*. *Who* would sound awkwardly, but would be more grammatical.

Letter XIX gives a definition of the ellipsis, which would be a lesson to Apollonius himself: the compasses, it seems, "do not take their sweep all round, but leave out parts of the area or surface." The objection to Blackstone's language is very questionable. "The very *scheme and model*, WAS settled," may, perhaps, be defended, because scheme and model are considered as one thing, the words being intended to illustrate each other, but not to point out different attributes of the administration of justice; and both words may be admitted, as a collective term, to govern a singular rather than a plural verb. It seems also to be an error to make *with* a conjunction rather than a preposition, and to say "The bag, with the guineas and dollars in it *were* stolen," or "zeal, with discretion, *do* much." "I expected to have seen," is justly noticed as a common error for "I expected to see." The meaning of an *active* verb is erroneously confounded with that of a *transitive* verb, in the remarks on the word *elope*, which means to go off, or to run off, and we should naturally say *was* gone off, but *had* run off.

The nature of the subjunctive mood is dismissed in the same letter without better success than has been obtained by former grammarians. An essay was published about thirty years ago in a periodical work, which brings the subject into a small com-

pass; suggesting that the subjunctive mood ought always to be considered as a *conditional future.* The examples given are, "If the Elbe *is now* open, we shall soon have the mails, and *then,* if there *be* any news from the army, I will send it you immediately." "If Catiline *was* generous, it was in order to serve his ambition." The subjunctive past, if I *were,* becomes present, by being the future of the past; going back to the time when the present was future, and therefore contingent; and this conditional sense involves no difficulty, except when a mistaken adherence to the fancied rules of grammar forces it in where it has no business: thus the rules of some grammarians would lead us to say, if Catiline *were* ambitious; which is totally contrary to the true sense of the subjunctive. Mr. Cobbett seems to have some such distinctions in view when he says that "*if* has nothing at all to do with the government of the verb. It is the sense which governs." By this he means that *if* does not require a subjunctive unless is relates to a *future contingency.* He is right in saying "Though her chastity *is* becoming, it gives her no claim to praise": but most decidedly wrong in adding "she would be criminal if she *was* not chaste"; for *was* is here used as relating to the present circumstances, which are the future of the past, and therefore require the subjunctive *were* to denote the condition intended. He has, however, done signal justice to the cause of this injured verb, by introducing it for *was,* in his sixth lesson, where he says it should have been "Your Lordship *were* apprized of every important circumstance."

Such errors as this, however, are easily corrected, and many of the acute remarks which have been here copied are well worthy the attention of practical grammarians; at the same time enough has been said, without any disparagement of Cobbett's talents, to show that a man cannot be well qualified to teach that which he has not had the means of properly learning. For although the English language appears at first sight to be extremely simple and philosophical in its structure, it has, in fact, been derived from a variety of heterogeneous sources; it has undergone a variety of vicissitudes, and has served for the expression of a multiplicity of discussions on the most refined subjects in literature and history and science, for

the feelings of oratory, and the passions of poetry, and it has been worn away by degrees, as the crystal in the stream is worn to a pebble, till it has returned to a simplicity which wears the aspect of the immediate offspring of the Chinese or Egyptian or Mexican Hieroglyphics. But with all this, it has still some spots, some idioms, which invariable custom obliges us to retain; and which can only be distinguished from corruptions and vulgarisms by tracing their history through the different stages of its progress, including, of necessity, the corresponding idioms in the parent languages out of which it has arisen.

Believe me always, my dear Sir,

Your's very sincerely,

* * * *

Malaria: an Essay on the Production and Propagation of this Poison, and of the Nature and Localities of the Places by which it is produced, with an Enumeration of the Diseases caused by it, and of the Means of diminishing and preventing them, both at Home and in the Naval and Military Service. By J. Mac Culloch, M.D., F.R.S., &c. &c. Longman and Co. 1827.

Though we have given a place in our Journal to two articles on Malaria from Dr. Mac Culloch, we have thought it expedient to take some notice of his book under the form of a review; particularly as some matters have come under our cognizance, which may add some illustrations to this subject where the author appears to have been in a state of deficient information, or to have shunned the question for reasons which appear to us somewhat over refined.

We allude principally here to the localities and the facts, as they are now before us; circumstances and events which seem to us of the greatest importance, as enforcing the value of the details which he has collected, and as holding out warnings to the people respecting the preservation of their healths, in addition to those which the work before us has given in describing the soils or characters of ground in England from which this destructive poison is generated. And before we proceed to the analysis of his book, we shall state what those are, or at least a few of them, while wondering that he should have overlooked them, or regretting that any fancies should have prevented him from stating what would have been of so much utility.

It is notorious that, in the last autumn, the remittent fevers in various parts of the country amounted to a species of pestilence, such as has scarcely been known in England from this cause, or we might almost indeed say, from any other disease since the days of Sydenham. Wherever ague had ever existed, or even been supposed possible, in those places was this fever found: so that in all the well-known tracts in Lincolnshire, Norfolk, Suffolk, Kent, Essex, Sussex, Hampshire, and so forth, there was scarcely a house without one or more inhabitants under fever, while the event, as might be suspected, was a considerable mortality. In the parish of Marston, in Lincolnshire, for example, it amounted to 25 in 300 inhabitants; in some other places, it reached one in sixteen, one in thirteen, one in nine. And so extensive was its range, that even Hastings did not escape; while it should be almost superfluous to say that every other town on the sea-coast was so much infested by it, that they who resorted to them for bathing, as usual, found themselves most awkwardly situated, and also suffered in considerable numbers.

To come nearer home, and to what must interest us of the metropolis more, the same fevers were extremely abundant in various parts of the outskirts of London, as also in the villages or towns which are connected with it, within a range of from six to ten miles. Not to enumerate all these, this was the case throughout the range of streets or houses which extends from Buckingham Gate to Chelsea; in which long line, it is said, that almost every house had a patient or more under this fever: though, as the author has truly observed, these were mistaken for typhus, or at least thus misnamed. Thus it was also about Vauxhall and Lambeth; and to a great extent among all that scattered mixture of town and country which follows from Whitechapel, from Bishopsgate, and so forth, and very particularly along Ratcliffe Highway, and so on, to an indefinite range along the river, not only on this side but on the opposite one, so as to include Rotherhithe, and then proceeding onward to Deptford, Greenwich, Woolwich, Plumstead, so as to carry us beyond the boundary which we proposed to notice.

And in addition to the towns or villages which we have just named, we may enumerate Lewisham, in which we knew one house in which there were nine patients under this fever, which proved mortal to one. Dulwich, especially subject to this disorder, Fulham, Ealing, and the several other villages along the Thames, as far as Chertsey; and even Rich-

mond, where, as at Lewisham, there was one house known to us, inasmuch as being intimate friends, where ten individuals at one time were suffering under this disease.

We must not prolong this enumeration, since we might easily occupy a dozen of our pages with similar details, ranging, in fact, all over England; but we must still observe, that whatever was the pestilence last year, it promises to be much greater in the present one. This is easily judged from the manner in which the season has set in; but still more decidedly from the extraordinary prevalence of ague in the spring; since that which is intermittent fever then, will be remittent in the autumn, or rather, as the author has justly remarked, there will scarcely be a definite season of vernal intermittent, but the remittent will commence immediately, increasing in extent and severity as the summer advances, and promising to become, in the autumn, the greatest season of disease that England has known for this century.

As an example of this, it must suffice to enumerate two or three facts, while these are as satisfactory for our purpose as a thousand would be. The most general of these is, that ague is at this moment extremely abundant where it was formerly so little known as not to be noticed, and that where single cases used to occur, there are now hundreds. Thus has it prevailed at Fulham and Ealing, and in the outskirts of London, and even in the town itself; and thus does it so prevail at Greenwich, Deptford, and in the associated vicinity, that a medical friend informs us, that it comprises more than two-thirds of his entire practice, which is very extensive; whereas a few years ago he had rarely a patient in a year. Thus also in the Military Hospital at Woolwich, there were in the spring three hundred patients with this disease; while in former times, we are assured, that an ague was scarcely known once in five or six years.

These are a few of the facts within our knowledge, but not one in a thousand, which evince the necessity of the publication before us; a book which seems to have been singularly well-timed, in as far as its purpose is, by a dissection of the sources of malaria, to diminish the ravages of both these kinds of fevers. And in this view we consider it a work of very considerable utility, inasmuch as it points out all the needful circumstances, as to prevention, in great detail; while these seemed particularly called for in England, from the entire and not less singular neglect which this subject has experienced, not only from the people at large, but from the medical profession. Beyond this, all that we need say of

the character of the work is, that it contains the only regular and complete attempt at the natural history of Malaria that has been executed; since the several foreign writings on this subject are partial, or imperfect, or local in their investigations; and having said thus much, we shall proceed to give a brief analysis of its form and matter. And this analysis may be truly brief, without inconvenience; since the two Essays from the pen of the author, to which we have given a place in our Journal, will supersede the necessity of making that useful and practical abstract which we should otherwise have felt ourselves bound to give.

To pass over an introductory chapter of the usual necessity, the author commences by pointing out the several disorders, in a general way, which are produced by malaria, for the purpose of proving the sources of this poison; and as we are of those who take the facts as already proved, we need not notice it further.

The third chapter details the characters of those soils or situations which are most commonly or generally admitted to produce this poison: and though it contains some facts not very universally known, we shall also pass it over as of less moment than that which follows.

This is the fourth chapter, containing the details of the circumstances producing malaria, which have been either denied or overlooked; and it is one of the most important practical chapters in the book, inasmuch as it is to the popular ignorance of these that we must attribute a large proportion of the cases of fever occurring in common life. These, therefore, we shall mark briefly; and even the briefest notice will be of use in the way of precaution, while we must refer to the book itself for those proofs of the truth of the several views which we could not take room to give. Generally, however, we may state this leading argument of the author, because it is brief, and, to us, appears satisfactory. It is this: that as the quantity of the poison which any person can inspire is necessarily small, and as this small quantity can be produced by a small marshy spot as well as a large one, it is the same, as to the production of disease, whether the marsh is a foot square or a mile, provided the exposure be complete: while also, any piece of ground where vegetables decompose under the action of water, is virtually a marsh, or must produce malaria.

This enumeration, therefore, under that view, comprises, in addition to marshes, whether fresh or salt, all the cases where water is present in such a manner as to act upon vegetables; and the chief are the following.

It is shown, and by facts, that the rushy swamps of high moorlands, however small the extent, do produce this disease; and we must not here forget to name what, however, belongs to the preceding chapter, woods and coppices, little suspected in England, yet shown to be the cause of fevers in Wales, and also in Sussex; very probably, every where else. It is also shown that meadows and moist pastures, whether in flat lands or on elevations, generate fevers; and very particularly, should they have been affected by inundation or unusual moisture, and if that should be followed by heat. And while it is also specifically shown how, in all cases, it is the produce of the drains or ditches required in meadow lands, it is distinctly proved that, even without these, malaria is produced, or that it is generated by the meadow or moist pasture itself.

It is also shown that this poison is produced by rivers, by all flat rivers at least, or those of which the progress is slow and through meadow lands; while this is pointed out as one of the causes, especially, which is not suspected or not believed in England. And here we can add a fact to our author's statement, which is decisive: this is the case of the barracks at Morne Bruce, in Dominica, situated on a steep and rocky hill, perfectly dry, and free from all other causes of suspicion, while eternally subject to the most severe fevers. And the cause is, a mountain stream, about 300 yards below this building, in the valley, always covered by a mist in the evenings, and ascertained, by direct experience, to be the very cause of the diseases in question.

Our author also notices canals, mill-ponds, ornamental waters, and all other pools and ponds, even to so small a dimension as those formed in gravel-pits; pointing out those, in particular, as common causes of fever about London, and apparently much inclined to pass a very severe judgment on the canal in St. James's Park, and also on the pond in St. James's Square, while apparently restrained by his prudential reasons, which appear to us sufficiently misplaced, or, as we should fairly call them, somewhat absurd. But as we must not affront a writer whose papers we have admitted, we shall say no more on this matter. In noticing drains, he also speaks of moats and modern fortifications; attempting to show that the fevers so common in the sieges of ancient castles were produced by their moats, and noticing the familiar fact of the frequency of fevers in fortified towns. Lakes also are pointed out as situations generating this poison: and it is here especially noticed that if, in those and other cases, malaria is produced by the vegetable growth and decompo-

sition, so is it the consequence of the exposure of the mud of such receptacles of water; a cause which is again treated of at greater length in the subsequent chapter.

This chapter relates to what the author calls obscure and disputed cases. We shall pass over these, which, as not implying precautionary measures, are of the least interest, and commence by noticing the case of vegetable putrefaction. It is attempted to show, that the vegetable need not be living to produce malaria, but that, even if utterly decomposed, its elements, acting on water, can generate this poison. Among the cases under this head, are flax and hemp ponds, common sewers and drains, dunghills, and tide harbours; and the evidences under each are sufficient to make good the assertion. But the most important of all, in our view at least, is bilge-water: since our author has pretty clearly shown that all the fevers of ships (excepting, of course, a few casual instances of contagion) arise from this cause, and that if ships were kept clean, fever or sickness would be nearly unknown at sea. This we do indeed conceive one of the most important points in the work before us; and if the author has referred to Sir Henry Baynton, as a stranger, we can quote him, as a friend, that warrants for all that is here asserted, and for far more; since his collection of facts on this subject is most important, and we think him almost culpable in not having long ago given them to the public. If the Leviathan was always the healthiest ship in the navy; if she even left the West Indies, after a long anchorage and service, with a crew of 500 men, and not one sick, it is a case in the navy which never occurred before, nor since, and which arose entirely from the knowledge of this able and careful officer respecting the subject that we are discussing.

A sixth chapter explains, under the head of revolutions in the production of malaria, a variety of circumstances not easily admitting of abridgment. The chief of these are, the effects produced by drainages, and reversely, those which arise from inundations or other incidental causes affecting the state of the soil. But the most important view which it contains is that which relates to the effect of embankment in rivers, and to the geological changes produced by the distribution of alluvia. As, however, we cannot well state this in a small space, we shall pass to the chapter on the Propagation of Malaria.

This is the largest, and, as it strikes us, the most interesting of the whole; while the author has made it the depository of a variety of remarks and recommendations on this

subject, very particularly as it relates to the army. If he is correct,—and we see no reason to doubt it, from the nature of the statements,—the ignorance of this subject, even among the medical department of the army, has been most extraordinary and most unaccountable; while if Walcheren is proof enough of this, the writer before us has pointed out facts enough to show that it was not a solitary case, while evidently restrained by fear of some sort—we are almost inclined to call it cowardice—from telling all that he might have told. And we do think it wrong to retain or suppress that which is important to the public safety, under a fear that the feelings of individuals may be hurt; since the business of a writer is with justice and utility, and the security or welfare of thousands is of infinitely greater moment than the comforts of a few, and those also culpable.

Under this head, propagation, the author describes how this poison is conveyed by the winds, while the facts add much to the number and variety of the precautionary measures. And here also we find a speculation of no small curiosity, respecting the East wind, attempting to prove that wherever this is insalubrious or pernicious, it arises from its being the vehicle of malaria; while attempting also to prove that this substance can be conveyed from Holland to the coasts of England in that wind. We shall not pretend to give an opinion on this subject; and, since the author himself has noticed it in the paper printed in our present number, we shall suffer our readers to form their own judgments respecting it.

One also of the most curious facts mentioned in this chapter, is the singular limitation of malaria; and we must admit that the instance quoted as to the Chatham road is so remarkable as to be almost incredible; though, as we find that all the people agree in it, we cannot pretend to say it is not a fact. Indeed the facts of this nature, so familiar at Rome, are fully as inexplicable; so that all we can conclude is, that we are ignorant of the philosophy of this subject: no very great cause of surprise, unless it were proved that we could explain every thing else which belongs to meteorology.

In the eighth chapter we have an explanation of the effects of climate and seasons in the production of malaria; and while we need not analyse the facts which it contains, we may introduce in lieu of this, the explanations which its statements afford as to that recent increase of the diseases of malaria which we noticed at the commencement of this article. The last few years have been distinguished for an

uncommon prevalence of East winds, and to such a degree indeed, that we can find no meteorological records at all to be compared with the history of these years. And while the history of the intermittent and remittent, in London at least, from the time of Morton and Sydenham downwards, shows that all its periods of such diseases have been periods of East winds, it is not difficult to see how it acts as to both classes of marsh fever.* To London, in particular, it is the best conductor, propagating the malaria from all the moist lands to the eastward. To the East coast, if our author's theory is valid, it brings the malaria from Holland; and, moreover, as it forms our hottest summers, it causes our own climate to approximate more to the southern ones, and thus enables our lands to produce a greater quantity of malaria than in ordinary summers.

To pass from the eighth chapter, the ninth is a partial sketch of the geography of malaria; a chapter for which the author apologises, but which is nevertheless a very interesting collection of facts on a subject where a volume is, doubtless, a desideratum. And it would require a volume; while, in spite of our author's fears, we can really see no reason why such a statistical account of health should not be drawn up for England, when the utility of it is unquestionable. It is true that people cannot abandon their homes or change their residences, because their lots happen to be cast in an insalubrious country. But it is not less important to know what and where these dangers are; because, though the inhabitants may be compelled to abide, they can still correct much of the evil by the various modes pointed out, or avoid much of the hazard by resorting to the obvious precautions. To be ignorant, is to be exposed to the full evil: to know where it lies, is to know how and where to avoid it in numerous ways; since it will be found that by far the greater number of diseases occurring, were not necessary or unavoidable, but have been the result of ignorance as to the precise fact or spot which did produce the effect in question. And this we conceive to be the great use of the book before us; and that if ever it, or a code of rules founded on it, shall become popular, or form a *vade mecum*, particularly in the country, the effect will be to reduce most materially the quantity of disease, and very particularly that which is by far the most serious, the summer and autumnal fevers. On this ground, we should be glad to see a geography of malaria for England; and we do hope that it will be undertaken by some person of sufficient industry, and of more

courage than our author; while we cannot doubt that whoever attempts it would at least find it a profitable speculation. With these remarks we must pass over this chapter, as we could take no statement from it which would serve any useful purpose; though, as far as it goes, it will form a very useful guide to travellers on the continent of Europe, or to those who, as emigrants, are in search of a residence abroad.

The tenth chapter examines the inquiries which have been instituted into the chemical nature of malaria, leaving the question just where it was. In fact we, as chemists, do not believe that this science is yet in possession of the means required for analyses of this delicate nature; but we see no reason whatever why it should be despaired of, when chemistry has already, within a very few years, effected things which seemed far more impracticable and hopeless.

The eleventh and last chapter contains an enumeration of the diseases produced by malaria, presenting a most formidable list, and absolutely making us shudder in some of the details which relate to the worst parts of France and Italy. The representation here given of the average of life in these districts is particularly striking; while of the truth of all the facts, we can speak from personal knowledge. Our author has also noticed the effect of this poison on animals; showing that it is the cause of the noted epidemics in cattle, and also of the rot in sheep. If he will look into Livy, he will find a confirmation, which he appears to have passed by when quoting that author for epidemic seasons: this being, that in the same years in which epidemic "pestilences" appeared among the people, there was also a great mortality among the cattle.

We do not know what his own profession will say of his attempt, or rather proposal, to prove that the celebrated disease of the nerves called Tic Douleureux is the produce of malaria and a mode of intermittent fever; nor how they will receive his proposal to arrange Sciatica and Rheumatic pains, with many other local diseases, under this head. But this is not our affair: and as he has promised us two other volumes, on all the diseases which are produced by malaria, including these, we must wait with patience; knowing at least that he is a dealer in facts and not in hypotheses, and expecting, that even if he should fail to establish his point, he will try to do it, as he has been used to do in the other sciences which he has attempted, through the road of facts and evidence.

An Account of a new Genus of Plants called Reevesia. By John Lindley, Esq., F.L.S., &c. &c.

In a collection of dried specimens of plants sent to the Horticultural Society from China, by Mr. Reeves, are a few branches, with flowers, of a remarkable genus which is at present undescribed, but which is of so curious a nature, and of such importance with reference to the determination of some natural affinities, that I have thought it deserving immediate record; especially as drawings of the fruit, which have been subsequently obtained from the same indefatigable correspondent of the Society, render its history tolerably complete.

The *branches* appear to be fragments of an evergreen tree; they are slender, rounded, and smooth. The *nascent gemmæ* are covered with a dense rufous pubescence. The *leaves* are alternate, becoming, towards the extremities of the branches, opposite by approximation; their form is ovate-lanceolate acuminate, and in size they vary from three inches to nearly six in length; the surface, even of the youngest, is perfectly smooth on each side; their veins are inconspicuous, the lowest pair of venæ primariæ being divergent at an angle of about 40°, while the others spread outwards at an angle of 55° or 60°; the venæ arcuatæ and externæ are obscurely seen, but form together a number of rhomboidal spaces, equal in diameter to nearly one third of each side of the leaf; the proportion borne by the petiole to the lamina is variable, sometimes equalling one-fourth of the length of the latter, and not unfrequently being less than one-sixth of its length: this proportion not depending upon the station of the leaves; the petiole is smooth, half-round, and thickened at the extremity, where it unites with the lamina. *Stipulæ* are none. The *flowers* are greenish-white, in terminal thyrsoid compound racemes; the upper part of the *rachis*, and of its branches, is slightly protected by stellate pubescence; the *pedicles* are closely covered with pubescence of the same nature, and have one subulate downy deciduous bracteola at the base, and another towards the apex. The *calyx* is inferior, campanulate, tapering a little towards the base, densely clothed with stellate pubescence, bursting irregularly at the apex into

four or five ovate teeth, which are somewhat imbricated during æstivation, but which are separated by the growth of the petals long before the expansion of the flower; the veins of the calyx are remarkably reticulated, and when cut, a considerable quantity of mucilaginous viscid fluid is exuded. The *petals* are whitish-green, hypogynous, with a convolute æstivation; their *ungues* are spatulate, and as long as the calyx; their *laminæ* oblong, spreading flat, and then overlapping each other at the base; at the point of separation of the unguis and lamina is a small callus, and on each side a notch upon the margin. The *stamens* are seated upon a long, filiform, subclavate, smooth torus; the *filaments* are consolidated into a capitate five-toothed cup, nearly closed at the orifice, and on the outside of this cup are placed the *antheræ*, three to each tooth; the latter are two-celled, with divaricating cells, which open longitudinally, and are so entangled with each other that the whole surface of the cup appears, when the antheræ have burst, to consist of a single many-celled anthera. The *pollen* is spherical and smooth. he *ovarium* is seated within the cup of stamens, and is so entirely concealed that it cannot be discovered till some part of the cup is removed by violence; it is ovate, smooth, and formed of five inseparable cells, each of which has two ovula placed one above the other, and attached to their placenta by their inner margin; the *stigma* is sessile, with five radiating lobes. From the Chinese drawing, the half-ripe fruit appears to be fleshy, with five deep angles, and five cells, without any remains of calyx, and with a slight appearance of separation between the lobes. The ripe fruit is an obovate, five-angled, five-celled, five-valved, retuse, woody capsule, with a loculicidal dehiscence, and no separable axis. The seeds are attached one to each side of the valves, and are expanded at their lower end into a wing.

From this description it is obvious that, with the single exception of the contents of the seed, we are in possession of all that it is essential to know of the structure of this plant. The next subject of consideration is its affinity.

The stellate pubescence, the thickening of the petiole at the point where it expands into the lamina, the station of the stamens upon a long filiform torus, the external position of the

antheræ, and the union of the filaments by threes into a cup surrounding the ovarium, are all characters that forcibly call to recollection the genus Sterculia. The calyx, indeed, in that genus is generally divided much more deeply than in the plant now under consideration, and the antheræ are usually seated at the base of the ovarium; but, on the other hand, in Sterculia colorata of Roxburgh, which, if a distinct genus, (ERYTHROPSIS) as I am inclined to believe, is nevertheless next of kin to Sterculia, the calyx is of the same figure and divided in the same degree, and the antheræ are also combined in a capitate cup inclosing the ovarium. If, however, we pursue this comparison further we find that, with the characters now adverted to, the similarity ceases; in Sterculia there are no petals, the calyx has a valvular not imbricate æstivation, the cells of the fruit separate into distinct folliculi, and do not combine into a solid woody capsule, and the seeds are destitute of wings.

The fruit suggests so obviously some affinity with Pterospermum, that it is next necessary to institute a comparison with that genus. Stellate pubescence, a calyx divided into five portions, five hypogynous unguiculate petals, and fifteen fertile stamens united into a cup, seated on a stipitiform torus, and surrounding the ovarium, a five-celled ovarium, a woody five-celled capsule, with a loculicidal dehiscence, no axis, and winged seeds; all these characters are common to Pterospermum and our plant; but on the other hand the points in which they differ are of much importance. The æstivation of Pterospermum is valvate recurved not imbricate; its calyx is five-parted, not four—five-toothed; its anthers have parallel not divaricating cells, and are seated upon long distinct filaments, not sessile, upon the outside of a capituliform cup; and finally the petioles of the leaves are not connected with the lamina by a thickened space. The seeds are also winged at the apex, not at the base, but upon this point it is not my wish to insist.

If the comparison thus instituted with Pterospermum and Sterculia be attentively considered, we cannot fail to remark that the subject of these observations is nearly equally related to both; to Pterospermum in its petals and fruit, to Sterculia in its calyx and stamens. It must, therefore, be stationed between those two genera, thus confirming the propriety of M.

Kunth's combination of the Sterculiaceæ of Ventenat with the Byttneriaceæ of Mr. Brown; and, in fact, breaking down every barrier between them.

There are many other points that will suggest themselves to the Botanist, in which this plant is highly worthy of consideration, but for the present it will be enough to give the botanical characters with which it may stand recorded. It is named in honour of John Reeves, Esq., now resident at Canton, to whom we are indebted for our knowledge of it, from whose unwearied exertions in the cause of science the botany of China has received material assistance, and to whom our gardens are indebted for many of the fairest ornaments they contain.

REEVESIA.

Ord. Nat. Byttneriaceæ; *Sterculiam (Erythropsin) inter et Pterospermum.*

Calyx campanulatus, 5-dentatus, æstivatione imbricatâ, pube stellatâ tomentosus, bracteolatus. Petala 5, hypogyna, unguiculata, æstivatione convoluta, callo inter unguem et laminam. Stamina in toro longo filiformi insidentia. Antheræ 15, sessiles, in cyatho capituliformi, apice tantum pervio, obsoletè 5-dentato connatæ, extrorsæ, biloculares, loculis divaricatis intricatis, longitudinaliter dehiscentibus. Pollen sphæricum glabrum. Ovarium sessile, intrà cyathum antheriferum, ovatum, glabrum, 5-angulare, 5-loculare, loculis dispermis. Ovula margini loculorum unum super alterum affixa, superiore basi concavo in inferiorem incumbente. Stigma 5-lobum, simplicissimum, sessile. Capsula stipitata, lignosa, obovata, 5-angularis, 5-locularis, loculicidò 5-valvis, axi nullo. Semina cuique loculo duo basi alata.——Arbor (Chinæ) foliis alternis exstipulatis, racemis terminalibus compositis; floribus albis.

1. Reevesia thyrsoidea.

Habitat in China (v. s. sp. in Herb. et iconem in Bibliotheca Soc. Hort.)

ASTRONOMICAL AND NAUTICAL COLLECTIONS.

i. *Elementary View of the* Undulatory *Theory of* Light. *By* Mr. Fresnel.

[Continued from the last Number.]

I shall not undertake to explain here in detail the reasons and the calculations which lead to the general formulas that I have employed to determine the position of the fringes and the intensity of the inflected rays: but I think it right to give at least a distinct idea of the principles on which this theory rests, and particularly of the principle of *interference*, which explains the mutual action of the rays of light on each other. The name of interference was given by Dr. Young to the law which he discovered, and of which he has made so many ingenious applications.

This singular phenomenon, so difficult to be satisfactorily explained in the system of emanation, is on the contrary so natural a consequence of the theory of undulation, that it might have been predicted from a general consideration of the principles of that theory. Every body must have observed, in throwing stones into a pond, that, when two groups of waves cross each other on its surface, there are points at which the water remains immoveable, when the two systems are nearly of the same magnitude, while there are other places in which the force of the waves is augmented by their concurrence. The reason of this is easily understood. The undulatory motion of the surface of the water consists of vertical motions, which alternately raise and depress the particles of the fluid. Now, in consequence of the intersection of the waves, it happens, that at certain points of their meeting, one of the two waves has an ascending motion belonging to it, while the other tends at the same instant to depress the surface of the liquid: consequently, when the two opposite impulses are equal, it can neither be actuated by one nor the other, but must remain at rest. On the contrary, at the points in which the motions agree in their direction, and conspire with each other, the liquid, urged in the same direction

by each of the forces, is raised or depressed with a velocity equal to the sum of the effects of the two separate impulses, or to the double of either of them taken singly, since they are now supposed to be equal. Between these points of perfect agreement and complete opposition, which exhibit, one the total absence of motion, the other the maximum of oscillation, there are an infinity of intermediate points, at which the alternate motion takes place with more or less of energy, accordingly as they approach more or less to the places of perfect agreement, or of complete opposition of the two systems of motion which are thus combined, or superinduced on each other.

The waves which are propagated in the interior of an elastic fluid, though very different in their nature from those of a liquid like water, produce mechanical effects by their interference, which are exactly of the same kind, since they consist in alternate oscillatory motions of the particles of the fluid. In fact, it is sufficient that these motions should be oscillatory, that is, that the particles should be carried by them alternately in opposite directions, in order that the effects of one series of waves may be destroyed by those of another series of equal intensity; for, provided that the difference of the route of the two groups of waves [derived from the same origin] be such, that for each point of the fluid the motions in one direction, belonging to the first series, correspond to the motions, belonging to the second, in the opposite direction, they must perfectly neutralise each other, if their intensity is equal: and the particles of the fluid must remain in repose. This result will always hold good, whatever may happen to be the direction of the oscillatory motion, with regard to that in which the undulations are propagated; provided that the direction of the oscillatory motion be the same in the two series to be combined. In the waves which are formed on the surface of a liquid, for example, the direction of the oscillation is [principally] vertical, while the waves are propagated horizontally, and consequently in a direction perpendicular to the former; in the undulations of sound, on the contrary, the oscillatory motion is parallel to the direction of the propagation of the sound, [or rather is

identical with it]; and these undulations, as well as the waves of water, are subject to the laws of interference.

The undulations formed in the interior of a fluid have here been mentioned in a general manner: in order to form a distinct idea of this mode of propagation, it must be remarked, that when the fluid has the same density and the same elasticity in every direction, the agitation produced in any point must be propagated on all sides with the same velocity: for this velocity of propagation, which must not be confounded with the absolute velocity of the particles, depends only on the density and elasticity of the fluid. It follows thence that all the points, agitated at the same instant in a similar manner, must be found in a spherical surface, having for its centre the point which is the origin of the agitation: so that these undulations are spherical, while the waves, which are seen on the surface of a liquid, are simply circular.

We give the name of *rays* to the right lines drawn from the centre of agitation to the different points of this spherical surface; and these rays are the directions in which the motion is propagated. This is the meaning of the term *sonorous rays* in acustics, and of *luminous rays* or *rays of light* in the system which attributes the phenomena of light to the vibrations of a universal fluid, to which the name of ether has been given.

The nature of the different elementary motions, of which each wave is composed, depends on the nature of the different motions which constitute the primitive agitation. The simplest hypothesis that can be entertained concerning the formation of the luminous undulations, is, that the small oscillations of the particles of the bodies, which produce them, are analogous to those of a pendulum removed but little from its point of rest; for we must conceive the particles of bodies, not as immoveably fixed in the positions which they occupy, but as suspended by forces which form an equilibrium in all directions. Now, whatever the nature of such forces may be, as long as the displacement of the particles is but small in proportion to the extent of their sphere of action, the accelerating force which tends to restore them to their natural position, and which thus causes them to oscillate on each side of it, may always, without sensible error, be considered as propor-

tional to the magnitude of that displacement: so that the law of their motion must be the same as that of the motion of the pendulum, and of all small oscillations in general. This hypothesis, which is suggested by the analogy with other natural phenomena, and which is the simplest that can be formed respecting the vibrations of the luminous particles, may be considered as experimentally confirmed by the observation, that the optical properties of light are all independent of any circumstances which cause the greatest difference in the intensity of the vibrations: so that the law of their motion must be presumed to be the same for the greatest as for the smallest.

It follows from this hypothesis respecting the small oscillations, that the velocity of the vibrating particle at each instant is proportional to the sine of an arc, representing the time elapsed from the beginning of the motion, taking the circumference for the whole time required for the return of the particle to the same point, that is, the time occupied by two oscillations, the one forwards and the other backwards. Such is the law according to which I have calculated the formulas which serve to determine the effect of any number of systems of waves of which the intensities and the relative positions are given. These formulas will be found in the Annals of Chemistry, vol. xi., page 254: [they may be applied with security to the phenomena there considered, though the perfect accuracy of the hypothesis in all possible cases may be questioned, upon the grounds of the microscopical observations on the motions of vibrating chords, published by Dr. Young in the Philosophical Transactions for 1800. TR.] Without entering into the details of the calculations, I think it necessary to show in what manner the nature of the undulation depends on the kind of motion of the vibrating particles.

Let us suppose, in the fluid, a little solid plane which is removed from its primitive position, towards which it is urged by a force proportional to the distance. At the beginning of its motion, the accelerative force produces in it an infinitely small velocity only; but its action continuing, the effects become accumulated, and the velocity of the solid plane goes on continually to increase, until the moment of its arrival at

the position of equilibrium, in which it would remain, but for the velocity which it has acquired ; and it is by this velocity only, that it is carried beyond the point of equilibrium. The same force which tends towards this point, and which now begins to act in a contrary direction, continually diminishes the velocity, until it is completely annihilated ; and then the force continuing its action produces a velocity in the contrary direction, which brings the plane back to its place of equilibrium. This velocity again is very small at the commencement of the return of the particle, or plane, and increases by the same degrees as it had before diminished, until the instant of the arrival of the particle at the neutral point, which it passes with the velocity previously acquired : but when it has passed this point, the motion is diminished more and more by the effect of the force tending towards it, and its velocity is reduced to nothing when it arrives at the place of the commencement of the motion. It then recommences, at similar periods, the series of motions which have been described, and would continue to oscillate for ever, but for the effect of the resistance of the surrounding fluid, the inertia of which continually diminishes the amplitude of its oscillations, and finally extinguishes them at the end of a longer or shorter time, according to circumstances. [It must not be inferred from this explanation, that the particles of a fluid transmitting an undulation have any tendency to vibrate for ever : on the contrary it has been admitted by the best writers on the theory of sound, that all the motions which constitute it, as considered in a fluid, are completely transitory in their nature, and have no disposition to be repeated after having been once transmitted to a remoter part of the fluid. Tr.]

Let us now consider in what manner the fluid is agitated by these oscillations of the solid plane. The stratum immediately in contact with it, being urged by the plane, receives from it at each instant the velocity of its motion, and communicates it to the neighbouring stratum, which it forces forwards in its turn, and from which the motion is communicated successively to the other strata of the fluid ; but this transmission of the motion is not instantaneous, and it is only at the end of a certain time that it arrives at a deter-

minate distance from the centre of agitation. This time is the shorter, as the fluid is less dense, and more elastic; that is, composed of particles which possess a greater repulsive force. This being granted, let us assume, in order to facilitate the explanation, the moment when the moveable plane is returned to the initial situation, after having performed two complete oscillations in opposite directions: at this moment, the nascent velocity, which it had at first, is transmitted to a stratum of the fluid removed from the centre of agitation by a distance which we may represent by d. Immediately afterwards, the velocity of the moveable plane, which has a little augmented, has been communicated to the stratum in contact with it: "hence *it* has passed successively through all the following strata;" and at the moment when the first agitation arrives at the stratum of which the distance is d, the second has arrived at the stratum immediately before it. Continuing thus to divide, in our imagination, the duration of the two oscillations of the moveable plane into an infinity of small intervals of time, and the fluid comprehended in the length d, into an equal number of infinitely thin strata, it is easy to perceive, by the same reasoning, that the different velocities of the moveable plane, at each of these instants, are now distributed among the corresponding strata; and that thus, for example, the velocity which the plane possessed at the middle of the first oscillations in the direction of the motion, must have arrived, at the instant in question, at the distance $\frac{3}{4}\,d$: so that it is the stratum at this distance which possesses at the moment the greatest direct velocity; and in the same manner when the plane arrived at the limit of its first direct oscillation, its velocity was extinguished, and the same absence of motion will be found at the distance $\frac{1}{2}\,d$.

It is always supposed, that the oscillations of the plane are so minute in comparison with the length d, that their extent may be neglected in this calculation: and this hypothesis is actually consistent with the fact, since there is every reason to suppose that the excursions of the incandescent particles are very small in comparison with the extent of an undulation, which, though an extremely minute space, is still an appreciable quantity, and may be actually measured. Besides,

even if the amplitude of these oscillations were not in the first instance so wholly inconsiderable, it would be sufficient to consider an undulation at a greater distance from the centre of agitation, in order that their extent might be diminished in any required proportion.

In the second, or retrograde oscillation, the plane, returning through the same space, must communicate to the stratum of fluid in contact with it, and to the rest in succession, a motion in a direction contrary to that of the first oscillation; for when the plane recedes, the stratum in contact with it, urged against the plane by the elasticity or the expansive force of the fluid, necessarily follows it, and fills up the vacuum which its retrograde motion tends to produce. For the same reason, the second stratum is urged against the first, the third against the second, and so forth. It is thus that the retrograde motion is communicated, step by step, to the most distant strata: its propagation is effected according to the same law that governs the direct motion; the only difference is in the direction of the motions, or, in the language of mathematics, in the sign of the velocities which are imparted to the molecules of the fluid. We see then that the different velocities which have existed in the solid plane, during its second oscillation, must exist at the moment which we are considering, in the different strata comprehended in the other half of d, but with contrary signs. Thus the velocity, for example, which the plane had in the middle of the second oscillation, which is its maximum of retrograde velocity, must now be found in the fluid stratum situated at the distance $\frac{1}{4}d$ from the centre of agitation, while the maximum of direct velocity is found, at the same instant, in the stratum which is at the distance $\frac{3}{4}d$ from the centre of agitation.

The extent of the fluid, agitated by the two opposite oscillations of the solid plane, is what we call the breadth of an *entire undulation*, and we may consequently give the name of *semiundulation* to each of the parts actuated by the opposite undulations; the whole constituting a *complete oscillation*, since it comprehends the return of the vibrating plane to the initial situation. It is obvious, that the two semiundulations, which compose the complete undulation, exhibit, in

the fluid strata which they contain, velocities absolutely equal in magnitude, but with contrary signs, that is to say, carrying the particles of the fluid in opposite directions. These velocities are the greatest in the middle of each of the semiundulations, and decrease gradually towards their extremities, where they entirely vanish: so that the points of rest, and of the greatest velocities positive and negative, are separated from each other by intervals of one fourth of an undulation.

The length of an undulation, d, depends on two things: first, on the promptitude with which the motion is propagated in the fluid; and secondly, the duration of the complete oscillation of the vibrating plane; for the longer this duration, and the more rapid the propagation of the motion, the greater will be the distance to which the first agitation has been extended at the instant of the return of the solid plane to its initial situation. If the oscillations are all performed in the same medium, the velocity of propagation remaining the same, the length of the undulations will be simply proportional to the duration of the oscillations of the vibrating particles from which they originate. As long as the vibrating particles continue to be subjected to the same forces, it follows from the principles of mechanics that each of their minute oscillations will occupy the same time, whatever their extent may be; so that the corresponding undulations of the fluid will continue to be of the same length; they will only differ from each other in the greater or less extent of the elementary vibrations of the particles, which will be proportional to the extent of the luminous particles; for it appears from what has already been stated, that each stratum of the fluid repeats exactly all the motions of the vibrating particle. The greater or less amplitude of the oscillations of the strata of the fluid determines the degree of absolute velocity with which they move, and consequently the energy, but not the nature of the sensation which they excite, which must depend, according to every analogy, upon the duration of the oscillations. It is thus that the nature of the sounds, transmitted by the air to our ears, depends entirely on the duration of each of the oscillations executed by the air, or by the sonorous

body which puts it in motion; and that the greater or less amplitude or energy of the oscillations only augments or diminishes the intensity of the sound, without changing its nature, that is, its tone, or pitch.

The intensity of the light must depend then on the intensity of the vibrations of the ether; and its nature, that is to say, the sensation of colour that it produces, will depend on the duration of each oscillation, or on the length of the undulation, the one of these being proportional to the other. [We find, however, nothing in light of the same colour that is at all analogous to the different register, quality, or *timbre* of a sound, by which, for instance, the sound of a violin differs from that of a flute in unison with it: the subordinate, or harmonic tones of the sound having nothing in light to correspond with them. Tr.]

The duration of the elementary oscillation remaining the same, the absolute velocity of the ethereal particles, at the corresponding periods of the oscillatory motions, is, as we have seen, proportional to its extent. It is the square of this velocity, multiplied by the density of the fluid, that represents what is called the living force in mechanics, or otherwise the energy or impetus of the particles, which is to be taken as the measure of the sensation produced, or of the intensity of the light: thus, for example, if in the same medium, the amplitude of the oscillation is doubled, the absolute velocities will also be doubled, and the living force, or the intensity of the light, will be quadrupled.

We must, however, take care not to confound this absolute velocity of the particles of the fluid with the velocity of the propagation of the agitation. The first varies according to the amplitude of the oscillations; the second, which is nothing but the promptitude with which the motion is communicated from one stratum to the other, is independent of the intensity of the vibrations. It is for this reason, that a weak sound is transmitted by the air with the same velocity as a stronger one; and that the least intense light is propagated with the same rapidity as the brightest. When we speak of the velocity of light, we always speak of the velocity of its propagation. Thus, when we say that light passes through 200 thou-

sand miles in a second, we do not mean, according to the undulatory system, that such is the absolute velocity of the ethereal particles; but that the motion communicated to the ether employs only a second to pass to a stratum at the distance of 200 thousand miles from its origin.

In proportion as the undulation becomes more distant from the centre of agitation, the motion, spreading over a greater distance, must be weakened in every part of the wave. It is shown by calculation, that the amplitude of the oscillatory motion, or the absolute velocity of the particles concerned in it, is inversely proportional to the distance from the centre of agitation. Consequently, the square of this velocity is inversely proportional to the square of the distance, and the intensity of the light must be inversely as the square of the distance from the luminous point. It must be remarked, that, for the same reasons, the sum of the living forces of the whole undulation remains unaltered; for, on one side the length of the undulation *d*, which may also be called its thickness, is invariable, and its extent of surface augmenting in proportion to the square of the distance from the centre, the quantity, or mass of the fluid agitated, is proportional to the same square: and since the squares of the absolute velocities are diminished in the same proportion as the masses have augmented, it follows that the sum of the products of the masses by the squares of the velocities, that is to say, the sum of the living forces, remains unaltered. It is a general principle of the motion of elastic fluids, that however the motion may be extended or subdivided, the total sum of the living forces remains constant; and this is the principal reason why the living force must be considered as the measure of light, of which the total quantity always remains very nearly the same, at least as long as it continues to pass through perfectly transparent mediums.

It may be remarked, that black substances, and even the most brilliant metallic surfaces, by no means reflect the whole of the light which falls on them; bodies which are imperfectly transparent, and even the most transparent, when of great thickness, absorb also, to use a common expression, a considerable portion of the light that is passing through

them : but it must not be inferred that the principle of living forces is inapplicable to these phenomena ; it follows, on the contrary, from the most probable idea that can be formed of the mechanical constitution of bodies, that the sum of the living force must remain always the same, as long as the accelerating forces tending to bring the particles to their natural positions remain unchanged, and that the quantity of living force which disappears in the state of light, instead of being annihilated, is reproduced in the form of heat.

In order to obtain a correct idea of the manner in which the oscillation of a small solid body occasions undulations in an elastic fluid, it has been only necessary to consider a complete oscillation of the solid plane, which produces an entire undulation. If we suppose the oscillations of the plane to be continually repeated, we shall have a series of undulations instead of a single one : and they will follow each other without intermission, provided that the vibrations of the particle first agitated have been regular. Such a series of regular and uninterrupted luminous motions I call a *system of undulations*.

It is natural to suppose, on account of the prodigious rapidity of the vibrations of light, that the luminous particles may perform a great number of regular oscillations in each of the different mechanical situations in which they are placed during the combustion or the incandescence of the luminous body, although these circumstances may still succeed each other in extremely short periods ; for the millionth part of a second is sufficient to exhibit, for example, 545 millions of undulations of yellow light ; so that the mechanical disturbances, which derange the regular succession of the vibrations of the luminous particles, or which even change their nature, might be repeated a million times in a second without preventing the regular succession of more than 500 millions of consecutive undulations in each state of the particle. We shall soon have occasion to apply this observation to the determination of the circumstances in which the interference of luminous waves is capable of producing sensible effects.

We have seen that each undulation produced by an oscillatory motion was composed of two semiundulations, which

occasioned in the particles of the fluids velocities exactly equal in their intensity, though opposite in the direction of the motions. Let us at first suppose that two whole undulations, moving in the same line and in the same direction, differ half an undulation in their progress: they will then be superinduced on each other through one half of their length, or of their breadth, as we should say in speaking of the waves of a liquid: but I here use in preference the term length as applied to the interval between the two points which are similarly affected by the motions of two consecutive undulations. In the supposed case of the coincidence of one half of each of the undulations, the interference will only take place with respect to the parts so coinciding: that is, to the latter half of the first undulation, and the preceding half of the second: and if these two semiundulations are of equal intensity, since they tend to give, to the same points of the ether, impulses directly opposite, they will wholly neutralise each other, and the motion will be destroyed in this part of the fluid, while it will subsist without alteration in the two other halves of the undulations. In such a case, therefore, half of the motion only would be destroyed.

If now we suppose that each of these undulations, differing in their progress by half the whole length of each, is preceded and followed by a great number of other similar undulations; then, instead of the interference of two detached undulations, we must consider the interference of two systems of waves, which may be supposed equal in their number and their intensity. Since, by the hypothesis, they differ half an undulation in their progress, the semiundulations of the one, which tend to cause in the particles of ether a motion in one direction, coincide with the semiundulations of the other, which urge them in the opposite direction, and these two forces hold each other in equilibrium, so that the motion is wholly destroyed in the whole extent of these two systems of waves, except the two extreme semiundulations, which escape from the interference. But these semiundulations will always constitute a very small part of the whole series to be considered.

This reasoning is obviously applicable to such systems only

as are composed of undulations of the same length; for if the waves were longer one than the other, however small their difference might be, it would happen at last that their relative position would not be the same throughout the extent of the groups; and while the first destroyed each other almost completely, the following ones would be less in opposition, and would ultimately agree completely with each other: hence there would arise a succession of weak and strong vibrations analogous to the beatings which are produced by the coincidence of two sounds differing but little from each other in their tone; but these alternations of weaker and stronger light, succeeding each other with prodigious rapidity, would produce in the eye a continuous sensation only.

It is very probable that the impulse of a single luminous semiundulation, or even of an entire undulation, would be too weak to agitate the particles of the optic nerve, as we find that a single undulation of sound is incapable of causing motion in a body susceptible of a sympathetic vibration. It is the succession of the impulse, which, by the accumulation of the single effects, at last causes the sonorous body to oscillate in a sensible manner; in the same manner as the regular succession of the single efforts of a ringer is at last capable of raising the heaviest church bell into full swing. Applying this mechanical idea to vision, supported as it is by so many analogies, we may easily conceive that it is impossible for the two remaining semiundulations, which have been mentioned, to produce any sensible effect on the retina; and that the result of such a combination of the two systems must be the production of total darkness.

If again we suppose the second system of undulations to be again retarded half an undulation more, so as to make the difference of the progress an entire undulation, the coincidence in the motions of the two groups will be again restored, and the velocities of oscillation will conspire and be augmented in the points of superposition; the intensity of the light being then at its maximum.

Adding another semiundulation to the difference in the progress of the two systems, so as to make it an interval and

a half, it is obvious that the semiundulations, superinduced on each other, will now possess opposite qualities, as in the case of the half interval first supposed: and that all the undulations must in this manner be neutralised, except the extreme three semiundulations on each side, which will be free from interference. Thus almost the whole of the motion will again be destroyed, and the combination of the two pencils of light must produce darkness, as in the case first considered.

Continuing to increase the supposed difference by the length of a semiundulation at each step, we shall have alternately complete darkness and a maximum of light, accordingly as the difference amounts to an odd or an even number of semiundulations: that is, supposing always that the systems of undulations are of equal intensity: for if the one series were less vivid than the other, they would be incapable of destroying them altogether: the velocities of the one series would be subtracted from those of the other, since they would tend to move the particles of the ether in contrary directions, but the remainders would still constitute light, though feebler than that of the strongest single pencil. Thus the second pencil would still occasion a diminution of the light: but the diminution would be the less sensible as the pencil is supposed to be weaker.

Such are the consequences of the principle of the interference of undulations, which agree perfectly, as we have seen, with the law of the mutual influence of the luminous rays which is deduced from experiment: for the results are expressed precisely in the same words, if we give the name of *length of undulation* to the difference of routes which had been represented by the symbol *d*. Admitting, therefore, as there is every reason to believe, that light consists in the undulations of a subtile fluid, the period *d*, after which the same effects of interference are repeated, must be the length of an undulation.

It appears from the table already given for the seven principal kinds of coloured rays, that this period *d*, or the length of the undulation, varies greatly, according to the

colour of the light, and that for the extreme red rays, for example, it is [more than] half as great again as for the violet rays situated at the other extremity of the spectrum.

It may easily be imagined that the number of different undulations is not limited to the seven principal ones which are indicated in the table, and that there must be a multitude of intermediate magnitudes, and others beyond the red and the violet rays: for the ponderable particles, of which the oscillations give rise to them, must be subjected to forces that are infinitely varied, in the combustion or the incandescence of the bodies which excite the motions of the ether: and it is on the energy of these forces that the duration of each oscillation depends, and consequently the length of the undulation produced by it. It is found that all the undulations comprehended [in the air] between the lengths .0000167 E. I. and .0000244, are visible; that is, are capable of exciting vibrations in the optic nerve: the rest are only sensible by their heat, or by the chemical effects which they produce.

It has been remarked, that when two systems of waves differ half an undulation in their progress, two of the semiundulations must escape from interference; that six must be exempt when the difference amounts to three semiundulations; and that, in general, the number of undulations exempt from interference is equal to the number of lengths of a semiundulation separating the corresponding points of the two systems. While this number is very small in proportion to that of the waves contained in each system, the motion must be nearly destroyed, as in the case of the exemption of a single undulation. But it may be imagined that, as we increase the difference of the progress of the two pencils, the undulations exempted from interference may become a material portion of each group, and that it may finally become so great as to separate the groups entirely from each other; and in this case the phenomena of interference would no longer be observable. If, for example, the groups of undulations consisted but of a thousand each, a difference of one-twentieth of an inch in their routes would be much more than sufficient to prevent the interference of the rays of all kinds.

But there is another much more powerful reason which prevents our perceiving the effects of the mutual influence of the systems of waves when the difference of their routes is considerable; which is the impossibility of rendering the light sufficiently homogeneous: for the most simple light that we can obtain consists still of an infinity of heterogeneous rays, which have not exactly the same length of undulation; and however slight the difference may be, when it is repeated a great number of times, it produces of necessity, as we have already seen, an opposition between the modes of interference of the various rays, which then compensates for the weakening of some by the strengthening of others; [while the shades of colour are not sufficiently distinct to allow the eye to remark the difference.] This is without doubt the principal reason why the effects of the mutual interference of the rays of light become insensible when the difference of the routes is very considerable, so as to amount to 50 or 60 times the length of an undulation.

It has already been laid down as one of the conditions necessary for the appearance of the phenomena of interference, that the rays which are combined should have issued at first from a common source: and it is easy to account for the necessity of this condition by the theory which has now been explained.

Every system of waves, which meets another, always exercises on it the same influence when their relative positions are the same, whether it originates from the same source or from different sources; for it is clear that the reasons, by which their mutual influence has been explained, would be equally applicable to either case. But it is not sufficient that this influence should exist, in order that it may become sensible to our eyes: and for this purpose the effect must have a certain degree of permanence. Now this cannot happen when the two systems of waves which interfere are derived from separate sources. For it is obvious that the particles of luminous bodies, of which the vibrations agitate the ether, and produce light, must be liable to very frequent disturbances in their oscillations, in consequence of the rapid changes which are taking place around them, which may

nevertheless be perfectly reconciled, as we have seen, with the regular continuance of a great number of oscillations in each of the series separated by these perturbations. This being admitted, it is impossible to suppose that these perturbations should take place simultaneously and in the same manner in the vibrations of separate and independent particles; so that it will happen, for example, that the motions of the one will be retarded by an entire semioscillation, while those of the other will be continued without interruption, or will be retarded by a complete oscillation, a change which will completely invert the whole effects of the interference of the two systems of undulations which originate from them; since if they had agreed on the first supposition, they would totally disagree on the second. Now these opposite effects, succeeding each other with extreme rapidity, will produce in the eye a continuous sensation only, which will be a mean between the more or less lively sensations that they excite, and will remain constant, whatever may be the difference of the routes described.

But the case is different when the two luminous pencils originate from a common source: for then the two systems of waves, having originated from the same centre of vibration, undergoing these perturbations in the same manner and at the same instant, undergo no changes in their relative positions: so that if they disagreed in the first instance at any given point, they would continue to disagree at all other times; and if their motions cooperated at first, they would continue to agree as long as the centre of vibration continued to be luminous: so that in this case, the effects must remain constant, and must therefore be sensible to the eye. This is therefore a general principle, applicable to all the effects produced by luminous undulations; that in order to become sensible, they must be permanent.

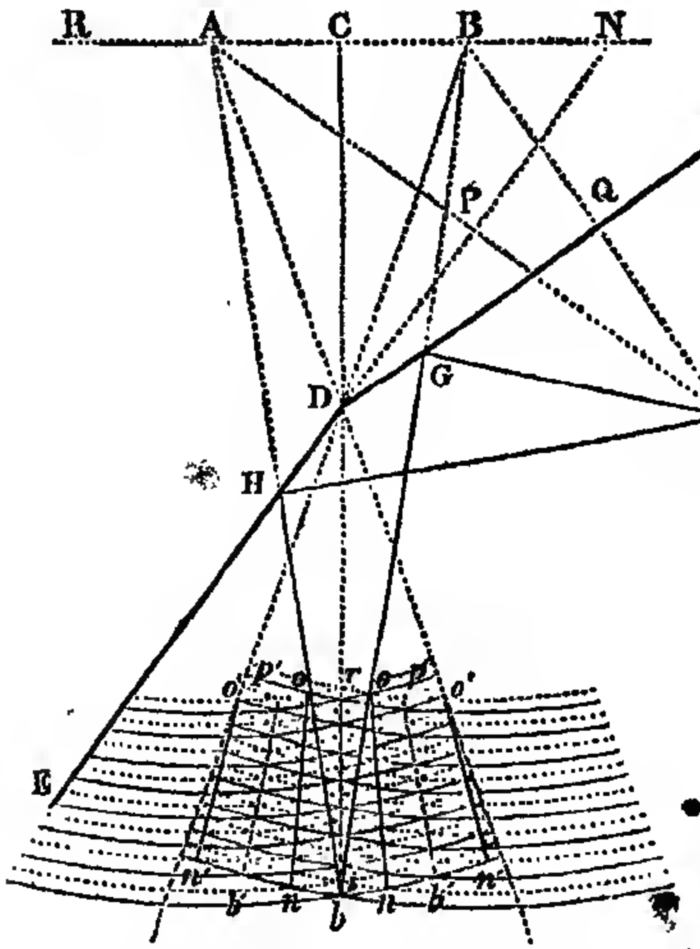
We have hitherto supposed that the two systems of waves were moving exactly in the same direction, and that consequently their elementary motions, to be combined with each other, were precisely limited to one single line: this is the simplest case of interference, and the only one in which the one motion can be completely destroyed by the other:

for in order that this effect may be produced, not only the two forces must be equal and in contrary directions, but they must also act in the same right line, or be directly opposed to each other.

The phenomenon of coloured rings, and that of the colours developed by polarised light in crystallised plates, present a particular case of interference, in which the undulations are exactly parallel. But in the phenomena of diffraction, or in the experiment with the two mirrors, which has been already described, the rays which interfere always form sensible though very small angles with each other. In these cases the impulses to be combined with each other at the same points, as belonging to the two systems of undulations, will also act in directions forming sensible angles with each other: but on account of the smallness of these angles, the result of the two impulses is almost exactly equal to their sum, when the impulses act in the same direction, and to their difference, when they are in contrary directions. Thus, in the points of agreement or disagreement, the intensity of the light will be the same as if the directions agreed more perfectly; at least the nicest eye will not be able to discover any difference in them. But although, with respect to the intensity of the light, this case of interference resembles that which has already been considered, there are other differences which modify the phenomenon very greatly, both with respect to its general form, and to the circumstances necessary for producing it.

We may take, as a convenient example, the case of diverging rays originating from the same luminous point, and reflected by two mirrors slightly inclined to each other, so as to produce two pencils meeting each other in a sensible angle: the two systems of waves will then meet each other with a slight inclination; and it follows from this obliquity, that if a semiundulation of the first system coincides perfectly in one point with a semiundulation of the second, urging the fluid in the same direction, it must separate from it to the right and left of the point of intersection, and must coincide, a little further off, on one side with the preceding semiundulation which is in a contrary direction,

and on the other side with the following semiundulation, and then be separated from this again, and at a distance twice as great as the first, must coincide with the second semiundulation before and behind it, of which the actions will coincide with its own: whence there will arise, on the surface of this undulation, a series of lines, at equal distances from each other, in which the motion is destroyed and doubled alternately by the action of the second series. Thus if we receive this luminous undulation on a white card, we shall observe on it a series of dark and bright stripes, if the light employed is homogeneous; or coloured fringes of different tints, if we employ white light for the experiment.



This will be more easily understood by the inspection of a figure, which represents a section of the two mirrors and of the reflected undulations, formed by a plane drawn from the luminous point perpendicularly to the mirrors represented by DE and DF. The luminous point is supposed to be S, and A and B are the geometrical positions of its two images, which are determined by the perpendiculars SA and SB falling from S on the mirrors, taking in them PA = SP.

and QB = SQ. The points A and B, thus found, are the centres of divergence of the rays reflected from the respective mirrors, according to the well known law of reflection. Thus, in order to have the direction of the ray reflected at any point G of the mirror *DF*, for example, it is sufficient to draw a right line through B and G, which will be the direction of the reflected ray. Now it must be remarked, that, according to the construction by which the position of B is found, the distances BG and SG will be equal, and thus the whole route of the ray coming from S and arriving at *b*, is the same as if it had come from B. This geometrical truth being equally applicable to all the rays reflected by the same mirror, it is obvious that they will arrive at the same instant at all the points of the circumference *n'bm*, described on the point B as a centre, with a radius equal to B*b*; consequently this surface will represent the surface of the reflected undulation when it arrives at *b*, or, more correctly speaking, its intersection with the plane of the figure: the surface of the undulation being understood as relating to the points which are similarly agitated at the same instant: the points being all, at the commencement of the whole oscillation, for example, or at the middle or the end, completely at rest; and in the middle of each semioscillation, possessed of the maximum of velocity.

In order to represent the two systems of reflected undulations, there are drawn, with the points A and B for their centres, two different series of equidistant arcs, separated from each other by an interval which is supposed equal to the length of a semiundulation. In order to distinguish the motions in opposite directions, the arcs on which the motions of the ethereal particles are supposed to be direct, are represented by full lines, and the maximum of the retrograde motions are indicated by dotted lines. It follows that the intersections of the dotted lines with the full lines are points of complete discordance, and of course show the middle of the dark stripes; and, on the contrary, the intersections of similar arcs show the points of perfect agreement, or the middle of the bright stripes. The intersections of the arcs of the same kind are joined by the dotted lines *b'p*, *br*, *b'p'*, and those of arcs of

different kinds by the full lines $n'o'$, no, no, $n'o'$: these latter representing the successive positions or the trajectories of the middle points of the dark stripes, and the former the trajectories of the bright bands.

It has been necessary to magnify very greatly in this figure the real length of the luminous undulations, and to exaggerate the mutual inclination of the two mirrors, so that we must not expect an exact representation of the phenomenon, but merely a mode of illustrating the distribution of the interferences, in undulations which cross each other with a slight inclination.

It is easy to deduce from geometrical considerations, that the length of these fringes is in the inverse ratio of the magnitude of the angle made by the two pencils which interfere, and that the interval, comprehended between the middle points of two consecutive dark or bright bands, is as much greater than the length of the undulation, as the radius is greater than the sine of the angle of intersection.

In fact the triangle bni, formed by the right line bi, and the two circular arcs ni and nb, may be considered as rectilinear and isosceles, on account of the smallness of the arcs; and the sine of the angle bni, considered as very small, may be called $\frac{ib}{bn}$: so that bn being the radius, ib will represent the sine of the angle bni, which has its legs perpendicular to those of the angle AbB: consequently, these angles being equal, one of them may be substituted for the other; and representing by i the angle AbB, formed by the reflected rays, we have $bn = \frac{ib}{\sin i}$; consequently nn, which is twice bn, will be equal to $\frac{2ib}{\sin i}$. But nn is the distance between the middle points of two consecutive dark stripes, and is the distance which has been called the breadth of a fringe; and ib being the breadth of a semiundulation, according to the construction of the figure, $2ib$ will be that of a whole undulation; consequently the breadth of a fringe may be said to be equal to the length of an undulation divided by the [numerical] sine of the angle made by the reflected rays

with each other, which is also the angle under which the interval AB would appear to an eye placed at b. We find another equivalent formula, by remarking that the two triangles, bni and $A\,b\,B$, are similar, whence we have the proportion $bn : bi = Ab : AB$, and $bn = \frac{bi \times Ab}{AB}$, or $2bn = \frac{2bi \times Ab}{AB}$: which implies that we may find the numerical breadth of a fringe by multiplying the length of an undulation by the distance of the images A and B from the plane on which the fringes are measured, and dividing the product by the distance of the two images.

It is sufficient to inspect the figure, in order to be convinced of the necessity of having the two mirrors nearly in the same plane, if we wish to obtain fringes of tolerably large dimensions; for in the little triangle bni, the side bi, which represents the length of a semiundulation, being little more than the hundred thousandth of an inch for the yellow rays, for example, the side bn, which measures the half breadth of a fringe, can only become sensible when bn is very little inclined to in, so that their intersection may be remote from ib; and the inclination of bn to in depends on the distance AB, which is the measure of the inclination of the mirrors.

If A and B, instead of being the images of the luminous point, were the projections of two very fine slits cut in a screen RN, through which the rays of light were admitted from a luminous point placed behind the screen in the continuation of the line bDC, the two paths described between the point and the slits A and B being equal, it would be sufficient to compute the paths described by the rays, beginning from A and B, in order to have the differences of their lengths; and it is obvious in this case, that the calculations which we have been making of the breadth of the fringes, produced by the two mirrors, would remain equally applicable, at least as long as each slit remained narrow enough to be considered as a single centre of undulation, relatively to the inflected rays which it transmits. It may therefore be said that the breadth of the fringes, produced by two very fine slits, is equal to the length of an undulation supposed

to be multiplied by the interval between the two slits, and divided by the distance of the screen from the wires of the micrometer employed for measuring the fringes.

This formula is also applicable to the dark and bright stripes which are observed in the shadow of a narrow substance, substituting the breadth of this substance for the interval which separates the two slits, as long as the stripes are far enough from the edges of the shadow: for when they approach very near to the edges, it is shown, both by theory and by experiment, that this calculation does not represent the facts with sufficient accuracy; and it is not perfectly correct in all cases, either for the fringes within the shadow, or for those of the two slits, but only for the fringes produced by the mirrors, which exhibit the simplest case of the interference of rays slightly inclined to each other. In order to obtain from the theory a rigorous determination of the situation of the dark and light stripes in the two former cases, it is not sufficient to calculate the effect of two systems of undulations, but those of an infinite number of similar groups must be combined, according to a principle which will shortly be explained, in treating of the general theory of diffraction.

ii. *Rule for the Correction of a* Lunar Observation. *By* Mr. William Wiseman, *of Hull.*

Rule.

Add together the reserved logarithm (found as directed, page 111 and 112 of the Appendix to the third edition of the Requisite Tables) the log. sines of half the sum, and half the difference of the apparent distance, and difference of apparent altitudes, and 0.3010300, the log. of 2. Then, to the natural number corresponding to the sum of these four logarithms, add the natural verse sine of the difference of true altitudes, and the sum will be the natural verse sine of the true distance.

Or, having obtained the natural number, as directed above, subtract it from the natural cosine of the difference of the true altitudes, and the remainder will be the natural cosine of the true distance.

Example.

(From page 112, Appendix to Requisite Tables.)

Reserved log. from Tables (Req.) 9th and 11th . .	9.9938860
Log. sin. 43° 23′ 5″ = ½ sum of app. dist. and diff. app. altitudes	9.8368895
Log. sin. 6° 45′ 36″ = ½ diff. ditto ditto	9.0708157
Log. of 2	0.3010300
Nat. num. to sum of 4 logarithms . . .1594488	9.2026212
Nat. vers. 37° 13′ 12″ = diff. true altitudes .2036812	
Nat. vers. 50° 26′ 28″ = true distance .3631300	

Or, Nat. cos. 37° 13′ 12″ = diff. true altitudes	.7963188
Nat. number found above	.1594488
Nat. cosin. 50° 26′ 28″ = true distance .	.6368700

Demonstration of the Rule.

Let M', S', D', d' and M, S, D, d, respectively denote the true and apparent altitudes, distances, and differences of true and apparent altitudes of the moon and sun (or a star); then will the theorem answering to the above rule be expressed by

$$\text{vers. } D' = \frac{2\cos M' \cos S'}{\cos M \cos S} \sin\frac{1}{2}(D+d)\sin\frac{1}{2}(D-d) + \text{vers.}\, d'.$$

By Bonnycastle's Trig. p. 175, the cosine of the angle contained by the co-altitudes is $\frac{\cos D - \sin M \sin S}{\cos M \cos S} = \frac{\cos D' - \sin M' \sin S'}{\cos M' \cos S'}$;

consequently the verse sine of the same angle

$= 1 - \frac{\cos D - \sin M \sin S}{\cos M \cos S} = 1 - \frac{\cos D' - \sin M' \sin S}{\cos M' \cos S'}$; that is,

$$\frac{\cos M \cos S + \sin M \sin S - \cos D}{\cos M \cos S} = \frac{\cos M' \cos S' + \sin M' \sin S' - \cos D'}{\cos M' \cos S'}.$$

Substituting cos d and cos d' for cos M cos S + sin M sin S and cos M' cos S' + sin M' sin S'. (Bon. Trig. p. 282), we have

$\frac{\cos d - \cos D}{\cos M \cos S} = \frac{\cos d' - \cos D'}{\cos M' \cos. S'}$; whence

$\cos D' = \cos d' - \frac{\cos M' \cos S'}{\cos M \cos S}(\cos d - \cos D)$; or, which is the same,

$\cos D' = \cos d' - \frac{\cos M' \cos S'}{\cos M \cos S}(\text{vers} D - \text{vers}\, d)$; or, (Bon. Trig. p. 286.)

$\cos D' = \cos d' - \frac{\cos M' \cos S'}{\cos M \cos S}(2 \sin^2 \frac{1}{2} D - 2 \sin^2 \frac{1}{2} d;)$ that is,

$\cos D' = \cos d' - \frac{2 \cos M' \cos S'}{\cos M \cos S} \sin \frac{1}{2}(D+d) \sin \frac{1}{2}(D-d)$; whence

also $\text{vers } D' = \text{vers } d' + \frac{2 \cos M' \cos S'}{\cos M \cos S} \sin \frac{1}{2}(D+d) \sin \frac{1}{2}(D-d)$.

It may be observed, that Requisite Tables 9—11, answer logarithmically to $\frac{\cos M' \cos S'}{\cos M \cos S}$; and the verse sines, and the cosines can be very readily taken out of the tables in the Appendix. Also no ambiguity can arise from the application of the rule before given: for all the arcs concerned in the operation will always be (each of them) less than a quadrant, except the resulting true distance, which cannot cause any ambiguity; and the verse sines are given in the Appendix, to 126°.

Example.

(*Example 2nd, p. 39, Requisite Tables.*)

Reserved log. from Tables 9 and 10		9.995307
Log. sin 62° 45′ 56″ = ½ sum app. dis. and diff. app. alts.		9.948971
Log. sin 40 43 31 = ½ diff. ditto ditto		9.814536
Log. 2		0.301030
Nat. num. corres.	1.147741	0.059844
Nat. vers. 22° 48′ 16″ = diff. true alts.	0.078167	
Nat. vers. 103 3 23 = true distance	1.225908	

De l'Influence des Agens Physiques sur la Vie. Par W. F. Edwards, D.M., Membre associé de l'Académie royale de Médicine de Paris, Membre de la Société Philomatique, de la Société de Médicine de Dublin, &c.

The researches of science among the phenomena of the physical world have long obtained a high degree of estimation and interest in general society; but it is of late years only that their application to living functions has attracted much of the attention of the literary world.

The laws which govern the action of animal organs (the proper department of Physiology) have usually been investigated by the medical profession, to which they especially

refer. Now we find that the public take some pains, and with reason, to inform themselves upon subjects connected with physiological knowledge. A well-educated person, disposed to philosophical inquiries, is not merely contented with the consciousness of living, and the common information he derives of its means by experience, but he seeks also to comprehend the relations subsisting between his own organisation and the matters with which he is surrounded, and which at once furnish him with nutrition, life, and support, and assail him with disease and annihilation. His own instincts and observation, joined to the more learned experience of his medical advisers, help him through the precarious stages of life, and these may perhaps be sufficient for all its purposes; and under this impression many will seek to know no more of the secrets of nature.

But we live in an inquiring and scrutinising age, when the demand for scientific principles is very generally urgent. All, therefore, relating to organisation seems of equal interest with that appertaining to what is termed the physical creation or inert matter.

Under this impression we have perused the book before us with great satisfaction, and propose to present our readers with an analysis of the valuable materials which it contains. We have some knowledge of Dr. Edwards, a countryman domiciliated in France, and long resident in Paris. We have confidence in his reports, and highly estimate his philosophical skill, extensive acquirements, and accuracy of observation, ranking him among the first physiologists of the age.

The work, now under consideration, contains an elaborate account of a long series of experiments, instituted for the purpose of ascertaining the influence of the physical agents upon animal life. These agents comprehend the atmospheric air, water, and temperature; the two first constituting the media in which all animals exist, and the last influencing in common the inhabitants of both media. It is true, this is a subject by no means new, for it has engaged the attention of experimenters from the earliest days of science. But Dr. Edwards has diligently and patiently sought to investigate the subject himself, to correct previous errors, and to embody the facts which he has accumulated into a more complete and regular system than heretofore adopted. In this attempt he has been eminently successful, and has effected more perhaps than all who preceded him, availing himself, nevertheless, of the experience of former inquiries.

The extent of his book, and the number of the experiments

are indeed somewhat appalling, but his clear and distinct method of arrangement greatly facilitates the reader's endeavours to master the extensive subjects of his pages. As a book of reference it should find a place in the library of every scientific society, and no individual devoted to philosophy should omit the possession of it.

The agency of the air around us, water, and heat and cold, have often been the objects of *chemical* inquiry, from their known great influence upon the animal economy. The changes effected by the phenomena of animal life upon these agents have been accurately examined, and partly reduced to a mathematical precision of calculation.

Spallanzani and others have viewed the subject as it regards physiology, but with such results as left the field open to subsequent investigation. Dr. Edwards seems to have seized upon the deficiencies of his predecessors, and, by going over their ground, and extending his own inquiries, he has arrived at most interesting and important results. These he has divided into four parts, as they relate to the different orders of the animal creation. The first part includes some of the lower animals, particularly tenacious of life, and of cold blood, such as frogs, toads, and salamanders. The second part is devoted to other animals of cold blood, and of the vertebrated order, as fish, and those reptiles which include lizards, snakes, and turtles. The third part refers to warm-blooded animals; and the fourth part of the work is dedicated to the influence of the physical agents upon the human race and vertebrated animals. To these the author has added the discoveries of modern times, relative to electricity on the animal economy, in an Appendix. A collection of tables is appended to the work, exhibiting the principal series of his experiments, as they regard the relative influence of physical agents on the duration of life, and the phenomena resulting from their mutual action.

The great importance of the four grand divisions of the work forbids our hastily reviewing them, and we will endeavour to condense so much of the information they contain as may forward the objects of our analysis. Dr. Edwards thus announces the arrangement of his work:—

"Ces recherches auront donc rapport à l'air dans les conditions de quantité, de mouvement et de repos, de densité et de raréfaction; à l'eau liquide et à la vapeur aqueuse; à la température, dans ses modifications de degré et de durée; à la lumière et à l'électricité. Ces causes agissent à la fois sur l'économie animale, ordinairement d'une manière sourde et imperceptible; et toujours

l'impression qu'on reçoit est le résultat de toutes ces actions combinées."

"Lors même que, par l'intensité de l'une d'elles, il nous arrive de distinguer la cause qui nous affecte, l'observation de l'effet se borne le plus souvent à la sensation, et les autres changemens qui l'accompagnent nous échappent. On conçoit par là que l'observation la plus attentive des phénomènes tels que la nature nous les présente, ne saurait démêler dans cette combinaison d'actions l'effet propre à chaque cause, ni reconnaître des effets qui ne seraient pas révélés par la sensation.

"Il est une méthode qui règle les conditions extérieures, qui fait varier celle dont on veut apprécier l'action, et qui fait juger, par la correspondance entre ce changement et celui qui survient dans l'économie, du rapport de cause et d'effet: c'est la méthode expérimentale; c'est celle que j'ai suivie. Pour en tirer parti il fallait, d'une part, déterminer l'intensité de la cause, d'autre part celle de l'effet. La physique nous fournit ordinairement les moyens de remplir la première indication."

In the true spirit of philosophical investigation, Dr. Edwards, in the first place, proceeds to examine the action of physical agents upon the simplest forms, and least elaborately developed organised beings, extending his inquiries upwards, in the scale of the animal world, to man, the most perfect creature, and the ultimate object of all physiological researches.

The peculiarity of constitution belonging to cold-blooded reptiles, among which there is so little mutual dependance of organs, renders these the best tests of the relative and proportionate influence of the different agents, the intense action of which is liable to destroy the more perfect animals; and the great development of the nervous system in the higher orders gives them a wider and more acute range of sensibility. It is difficult, at all times, and often impossible, to insulate corporeal functions among the warm-blooded classes, so as to ascertain the amount and limits of physical agency. The four classes of vertebrated animals, or such as are furnished with true spines, afford ample means of comparative illustrations; and these departments hav engaged the author's attention, in order to display the result of the action of the same agent exercising a uniform influence upon constitutions very differently constructed. The *air*, for example, exercises its influence uniformly upon he four mentioned classes of vertebratæ, and their different families are similarly exposed to the action of the atmosphere by respiration.

Curious and interesting as is this subject, it is singular

that, while it was among the first to be noticed, it has been the latest in producing satisfactory results. Among the opposing causes of the advancement of knowledge in this department, the ignorance of our ancestors in *chemical* science seems to be the principal. Without chemical aid it is perfectly useless to attempt the investigation. The composition of the air respired must be well understood; the different gases must be carefully examined, or the physiological inquiry will be darkened and obscured.

Dr. Priestley laid the foundation of our chemical knowledge of gases in their relation to respiration; but some time elapsed before it was understood in what manner the air was connected with animal organisation. Oxygen gas, one of the known constituents of atmospheric air, was Priestley's discovery, in its effect upon the blood, of converting this fluid from a dark purple to a bright crimson. Lavoisier founded a chemical theory upon this discovery of the agency of air, which was subsequently applied by Goodwin to physiology. The latter author demonstrated, by a series of excellent and correct experiments, that the exclusion of atmospheric air produces death in animals, in consequence of the dark-coloured blood usually circulating in the veins being prevented from becoming crimsoned. The state in which any animal may be thus placed, is known by the term ASPHYXY, and by which is to be understood a deficient or suspended aërification of the blood, from whatever cause it may proceed that the atmospheric air is prevented from access to the blood as it circulates through the lungs.

The great French anatomist, Bichat, pursued this subject still farther, and published a treatise on Asphyxy. He sought, by numerous experiments, to determine the threefold relation of the air to the nervous system, respiration, and the circulation; and he arrived at this great and important conclusion, that the VENOUS OR DARK BLOOD CIRCULATING THROUGH THE BRAIN, CREATES A CESSATION OF THE FUNCTIONS OF THAT ORGAN, AND THAT IN CONSEQUENCE THE HEART LOSES ITS ACTION. This discovery shows us at once the direct cause of asphyxy in all its different degrees, according, in effect, to the vitiated state of the blood from its deficient or suspended aërification.

Le Gallois also investigated the subject of asphyxy; and he found that, when *dark blood circulated through the spinal marrow, the motions of the heart ceased*; and thus he not only determined the relations of the nervous system to atmospheric air, but also those of the respiration and the circula-

tion, explaining the action of the air upon animals physiologically.

In this inquiry warm-blooded animals were almost exclusively referred to.

Spallanzani certainly investigated the action of the air on animals of cold blood, but less in relation to the three grand objects of Bichat and Le Gallois; and Spallanzani had the misfortune to live in an age when neither chemistry nor physiology had made such advances as the present age has produced.

Messrs. Humboldt and Provençal have, indeed, supplied much of this deficiency, by their researches into the respiratory functions of fishes. Nevertheless, the ground was still open, and our author has justly appreciated the extent of former inquiries, and observed that the phenomena of cold-blooded animals were too extraordinary to be noticed lightly, and required much more extensive observation than was previously bestowed upon them. With this impression, he proceeded to form an estimate of the comparative influence of the air and water upon the nervous and muscular systems of cold-blooded animals, which the singular modifications of life among reptiles in particular afford ample means of ascertaining.

We know that these animals possess the extraordinary property of existing a considerable time after the removal of the heart, with the free exercise of their senses and of voluntary motion, notwithstanding the suppression of the circulation. Dr. Edwards accordingly selected *salamanders* for his first investigations, and removed the heart, with the bulb of the aorta. Two of these were exposed to the free action of the air, and the other two were submersed in water previously deprived of air by boiling; a similar temperature being maintained in each medium. In four or five houra those submersed in the non-aeëated water ceased to be active, unless irritated, when they still appeared to retain voluntary power. One died in eight, and the other in nine hours. The salamanders in air lived from twenty to twenty-six hours and upwards. These comparisons were frequently repeated, and upon frogs and toads, with the same results, showing the experiments in air to be far more favourable to their existence than with the animals submersed in the water. Eight hours were about the maximum of the duration of life among the animals submersed in the water, and twenty-nine among those exposed to the air; so that, independently of respiration, the air is thus proved to be the most proper

medium for the action of their nervous and muscular systems, in their insulated state, the respiration and the circulation of the blood being both suspended. As a further corroboration of the superior vivifying property of the air over simple water, when the same animals were plunged into unaërated water during a certain time, as soon as they were removed into the atmosphere, they instantly revived; and their nervous and muscular systems were acted on according as they were placed in either medium. Dr. Edwards also confirmed the observation of Goodwin relative to the effect produced on the colour of the blood. Properly speaking, the asphyxy comes on the instant the air is excluded; the shades of difference in the colour of the blood being referrible to the air left in the lungs after cessation of respiration.

The next point to determine was the influence of the air upon the same animals exercising the respiratory function, and retaining their circulation, compared with those deprived of these functions.

The difference of time in the two cases developes the influence which the general circulation of the blood, free from aërial contact, exercises upon the nervous system.

To ascertain this point, an equal number of frogs, deprived of the power to exercise their respiratory and circulating functions, together with others left entire, were respectively plunged into disaërated water. At times the difference in favour of the untouched animals was twenty-four hours in favour of the duration of life. Similar trials with toads and salamanders produced the same results. In each case *asphyxy* came on; but the existence of the animals which lived without the respiration and circulation was much shortened. Thus the relative powers of life between the sole and insulated action of the nervous system, and its action combined with the circulation of dark blood, were estimated. The inference to be deduced, therefore, is, that although disaërated blood furnishes but an ephemeral sort of existence, it nevertheless exercises a comparatively favourable influence upon the nervous and muscular systems, since it tends to the prolongation of the action of these animal functions.

Dr. Edwards next proceeds to investigate the phenomena of *asphyxy* produced by *strangulation*, or the mechanical obstruction to the access of air to the lungs, and consequently to the blood. The same animals were employed. When the windpipe was rendered impervious by ligature, the muscles of the animals seemed to be paralysed directly; and although their motions became subsequently revived at times,

they never altogether recovered their perfect freedom. As a comparative illustration, an equal number of frogs were submersed in water, all of which died in about ten or eleven hours, while those which were strangled lived from one to five days. Salamanders continued active longest, and one did not cease to exist till the eleventh day, although during this time he was in a complete state of *asphyxy* from perfect strangulation.

Dumeril once found that a salamander lived a long time after decapitation, even when the cicatrix of the wound was healed so as to stop all access of air to the lungs.

In comparing the effects of strangulation with those of submersion or drowning, it is to be supposed either that these animals exist a limited period without the necessity of the nervous system being in contact with atmospheric air, or that the air influences their blood through the integuments of the body. Accordingly Dr. Edwards put this to the test by making experiments upon *cutaneous respiration*.

Spallanzani found that the exposure of cold-blooded animals to the air was attended with exudation of *carbon*, a phenomenon similar to that of respiration. There appears, however, to be some source of error in these experiments, for Spallanzani removed the lungs, and this operation rendered the animal liable to the absorption of air and loss of blood. Dr. Edwards sought to effect the same purpose by a different and more successful measure. He also confined frogs in vessels of atmospheric air, and fastened bladders round the head and neck, tight enough to stop the entrance of air to the lungs. At the expiration of two hours the air was examined in the bladder, and it was found to contain an excess of *carbonic acid*. The same result was obtained from salamanders. It appears, therefore, that while air is in contact with the skin, *carbon* is given out; but whether this be the effect of exhalation merely, or that *oxygen* is actually absorbed, and *carbon* transpired, is a question which led to further inquiries. Dr. Edwards, therefore, inclosed cold-blooded animals in *solid substances*, in order to determine the influence of dark-coloured blood, free of all external agency, in the production of chemical changes, and to observe its sensible effect upon the nervous system.

In the year 1779 three toads were confined in a box hermetically sealed, and so deposited in the Academy of Sciences. Eighteen months after, the box was opened, and one toad was found dead. These animals have been found alive in blocks of coal after an imprisonment of some years, and have

also been sealed up during similar periods without perishing. Possibly some hole or crevice might have admitted a little air. But, in Hevissant's experiment of 79, care seems to have been taken to obviate this suspicion.

Dr. Edwards, however, determined to put the question to the test. He enclosed ten out of fifteen frogs in thick wooden boxes, and filled the interstices with plaster, covering them over with the same substance, the toads lying each in a central hole or bed. The other five toads were at the same time submersed in water, and at the expiration of eight hours they were found to be dead. In sixteen hours more, one toad was taken from a box and found to be lively, and was reconsigned to its prison. On the sixteenth day the toads in the boxes were discovered alive, and thus the fact was established that these animals can live far longer in a state of asphyxy confined in solid substances, than when submersed in water. This was confirmed by repeated trials on salamanders, frogs, and toads. The frogs perished quickest.

Thus an extraordinary fact is established, as regarding reptiles, since it affords an exception to the general rule that *all animals require a* CONSTANT *supply of fresh air for the maintenance of their existence.*

Similar trials were repeated in sand, and with the same results.

Dr. Edwards found that although a certain quantity of air enters the boxes and sand, yet that it is far too little to maintain life. His conclusion, therefore, stands, that animals of the kind employed can live longer in *solid substances* than in a limited quantity of *dry air*.

It remains, however, to be considered in what manner these animals have their lives extended beyond those exposed to the action of a body of air. Dr. Edwards supposes the *moisture* of the sand to be one cause, since in the dry air the animals become *desiccated*, the cutaneous transpiration being lost in one case, and retained in the other, by the exclusion of air. A rapid and abundant transpiration from the body, united with deficiency of air, seems to be a greater cause of dissolution than confinement in solid substances wherein there is no waste by transpiration.

The author's inquiries are next directed to the influence of *temperature* upon animals of cold blood, and two and forty experiments are practised upon this subject, from the month of July to September following, during which period frogs were submersed in aërated water, with a view of settling the duration of life, acted on by varieties of temperature. The

continuance of life, generally, in these experiments, varied from one to two hours and twenty-seven minutes. The mean term of life was one hour and thirty-seven minutes, as averaged in July, and in September one hour and forty-five minutes, the two extremes of the seasons approximating the effects. The duration of the frog's existence was greatest in the greatest depression of temperature. Thus at ten degrees the duration of life was more than double what occurred at sixteen or seventeen degrees, and at zero it was about triple. As the heat was increased, the duration of life was diminished; at forty-two the frogs died, and in the lowest temperature they lived longest.

It appeared that at zero the frogs did not become stiffened, but retained their motion, and their resistance to the frozen state is the cause of the continuance of their existence at a low temperature. The cause of this resistance is to be found in their peculiarity of constitution. Toads produced similar results.

It may be alleged that frogs naturally live in climates at from forty to forty-two; but, it is to be observed, that they are then placed in a situation of liberty to come to the surface of the water to respire when they please; whereas in these experiments their respiration is limited, from their inability to reach the surface.

Taking a wider range of temperature, Dr. Edwards sought to ascertain the influence of the *seasons*. In July and September frogs were found to live from one to two hours and twenty-seven minutes in aërated water at fifteen and seventeen degrees. In November they died at the end of more than double this period, under the same temperature, and all other circumstances being similar excepting the season. As the autumn advanced life was prolonged.

To what are we to ascribe the modifications of the seasons? Probably to circumstances appertaining to the intensity of light, to electricity, to temperature, to the pressure of the atmosphere, to dryness and moisture, &c.? Such existing causes naturally suggest themselves. But it appears that little or no account can be rendered as to pressure, since its variations were too trifling during the two seasons. Moisture could not effect an influence, because the experiments were performed in water. The motion of the air was also obviated. Of all the suggested modifications *temperature* alone acted, and this, as it related to the surrounding air, was rendered ineffectual by artificial temperature. The animals, therefore, could only be affected as to the temperature of the

seasons *by that which preceded the experiments*. The modifications of the seasons, therefore, appeared to influence the cold-blooded animals used in the experiments in this point of view only. Accordingly we have this remarkable result, that the animals lived twice as long in autumn as in the summer preceding, when plunged in water of equal temperature. The *seasons* evidently influence their constitutions, so as to extend the duration of life independently of other causes, that is, from summer to autumn. Dr. Edwards endeavoured to ascertain if it proceeds from atmospheric temperature, and he found that frogs lived in aërated water at ten degrees, during November, from five or ten to eleven, and even to forty hours, in some instances, the last term being about double the duration of life in water of the same degree in summer. This proves the remarkable dependence of the frog's life under water, and the temperature of the month preceding. Two curious facts are thus developed by experiments instituted at different seasons. First, the influence of the temperature of the water in which the animals were placed; and secondly, the influence of the temperature of the air during certain periods preceding the experiments, for in autumn the duration of life was about double that of summer, and in winter he found the term to equal autumn, the temperature of the air being in each comparative experiment artificially raised to the same degree.

It appears from the foregoing experiments that frogs, toads, and salamanders, exist in water according to its *lowness of temperature*, and that their lives are prolonged *by the temperature which precedes the experiment being lowered.* It then becomes a question, what are the limits of this influence? This is to be ascertained by observing the greatest duration of life among animals deprived of external air by submersion in water; and noticing at the same time all the favourable circumstances dependent on the concurrent temperature in prolonging life among the cold-blooded animals.

A point relative to the natural history of frogs first presents itself to our notice. Spallanzani is of opinion that frogs do not pass the winter under water, but retire in October from their native rivers into moist sands, in which they make openings to breathe the air through, called by the Italian fishermen *il respiro della ranà*.

M. Bosc, and other French naturalists, found that frogs retire from October to spring *into water*, but they give us no direct proof that they constantly remain submersed. The presence of the observer may alarm the frogs, and thus pre-

vent their putting their heads above the water, so that the assertion is but a negative kind of proof that they remain so long under the water without coming up to respire, as some affirm. M. Bose declares he watched frogs approach the surface at regular periods every day during the winter season. Under the most favourable circumstances Dr. Edwards found that frogs could not remain submersed, in winter, more than two days and a half. Frogs are less active during winter than at the other seasons, but they never lose their motion. Were it true, as Spallanzani thinks it is, that they remained so long under water, it is probable that they would become frozen in winter and die. Spallanzani derives his opinion from what occurs with fish, forgetting that frogs are amphibious, and live as well on land as in water; whereas fish are limited to a watery medium, and can, therefore, furnish no example.

Dr. Edwards found that frogs, placed in certain quantities of aërated and non-aërated water of an equal temperature, lived longest in the *former*; but that the difference was not constant in its results, being often twice as long in one case as in the other, as to the duration of life.

The next inquiry regarded *stagnant water renewed at intervals*, and in this the duration of life was prolonged beyond the term of the last experiments, and even to eight days. During winter when the temperature was lowest the frogs remained active, though less so than in spring.

The conclusions to be drawn from these experiments are, that frogs pass the winter *in an animated state in water*, not becoming stiffened as in ice, and that they need not to approach the surface of the water in order to respire, provided the water they inhabit *be renewed at intervals;* but if the water be not renewed, or if disaërated water be employed, the frogs perish.

Considering that these animals are truly amphibious, these results are very curious; and it is interesting in a physiological point of view, to know that frogs are able to respire the air contained in the dense medium of water for an indefinite period, and just as easily as they breathe the finer medium on land.

Respecting the action of aërated water on the skin, the conclusion drawn seems to be correct, that it must be from *cutaneous* absorption that the air contained in the water prolonged the continuance of life in Dr. Edwards's experiments upon this point, since the animals were in a state of *asphyxy* regarding respiration by the lungs; and that no

air entered in combination with water was shewn from Dr. Edwards *never having seen water in the lungs.* Therefore, unless the air acted on the blood through some other organ, the lives of these animals would be definite and shortened, even though the water be renewed from time to time, and their *asphyxy* would be complete and continued. And since the skin is the only organ in contact with the air, it is fair to conclude that it is the medium of aërial absorption.

When the *webs* were examined under water, these membranes indicated the action of air upon their blood-vessels, by the bright tint of the blood.

Spallanzani imagined that frogs perish sooner in *running* than in *stagnant* water; but Dr. Edwards having secured some of these animals in ten feet of the Seine, whilst others were simultaneously placed in *unrenewed stagnant water*, he found the latter did not survive many hours, and the former lived a long time.

In order to fix the limits of this kind of existence, frogs were placed in *renewed aërated water*, and with a temperature never forced beyond ten degrees they were found to live *in all seasons of the year;* but when the temperature was elevated from twelve to fourteen, they died in a few hours. In running streams they lived longest, and at twelve degrees they were thus more favourably placed than in stagnant water, *at a lower temperature even*, and taking the precaution to renew the water daily; and at seventeen degrees in running water they died prematurely. Toads exhibited the same comparative results, but they lived the longest.

It appears, therefore, that water contained in vessels is less favourable to the lives of these animals than running streams, although the water and the temperature were identical. Probably the great advantage of running water is its *constant and unceasing renewal.* The separate and comparative influence of air, water, and temperature, being thus investigated, the combined action of the three physical agents was next inquired into, and it is demonstrated that frogs submersed in water are influenced by three circumstances,—1. the presence of *air* in water; 2. the quantity of its renewal; 3. the *temperature* of the medium. If the manners of frogs be closely examined, they appear to live in water under very considerable influence from the atmosphere.

From circumstances developed in the foregoing experiments, *cutaneous respiration* seems to be pretty evidently indicated. A chapter is, therefore, devoted to this subject, one that is not well known, although pulmonary respiration is

generally understood. In frogs, the function of pulmonary respiration is united with that of deglutition, and the air enters only by the nostrils, the mouth being closed during respiration. While the mouth remains open, the action of deglutition is stopped, and, therefore, the animal does not then breathe. Dr. Edwards availed himself of this circumstance by gagging the mouth so as to keep it open, and thus prevent the air from entering the lungs. The frogs were sufficiently exposed to moisture and renewal of air to their bodies: the results were, that, at twenty-four degrees, five frogs so placed died next day, and one lived a week.

Dr. Edwards immersed some frogs in wet sand, and adopted an improved method of excluding air from the lungs, and some of them lived twenty days. Hence it evidently appears that air influences the skin materially, and counterbalances the asphyxious state induced by obstructing the air's passage to the lungs. By adopting other methods, the existence of frogs was prolonged to thirty or forty days. It is, therefore, sufficiently proved that the blood undergoes its necessary changes from atmospheric influence through the medium of the skin, although in a minor degree compared with those which it passes through from pulmonary respiration. Frogs are thus shewn to possess a double source of respiration.

By substituting *oil* for water, frogs immersed in this fluid died in a few hours, being at liberty to breath the air on its surface. And, when plunged into oil, with the means of breathing by the lungs arrested, they lived an equal time with frogs simultaneously placed in water without power to respire. A comparison was instituted with frogs in oil and in water, being allowed to breathe air, when the difference was found to be very considerable in favour of the aquatic bath. These circumstances shew, that, even with the feeble succour of the air through the skin, absorbed from the water, the respiratory function was far more prolonged, than in the case of the obstruction afforded by the oil. Thus we have abundant evidence of the double function by which frogs are maintained, from the action of the air on the skin and the lungs; and this appears to be the means of existence among *amphibious animals generally*.

It may be asked why these animals die in deep water when prevented from approaching the surface? It appears that, having expelled the respired air from their lungs, which is imperfectly renewed from the water, they become specifically heavier than the water, and unable to rise from the bottom; and thus placed, the duration of their lives depends upon

the resistance offered by their constitutions to the depressing effects of a state of asphyxy while remaining submersed.

Dr. Edwards next proceeds to inquire into the effects of TRANSPIRATION. A liquid transfusion from the skin of animals is constantly going on, either in the form of vapour or of fluid in a denser state.

The latter constitutes sweat. This phenomenon exhibits great variations, and it is important to know what diminution of weight the body suffers in different circumstances. In the course of an hour remarkable fluctuations occur.

Dr. Edwards suspended frogs, toads, and salamanders, in a *calm air*, weighed them, and noted the results, which, though very changeable in an hour, were generally uniform in three, and in nine hours they averaged an equal result. The successive diminution in the mass of fluids was evident.

The results were modified by the alternate position of the animals in a body of air in repose, or agitated by a draft. And these results do not appear to depend upon any principle of vitality, for they take place equally in death and in life, and indeed among unorganized bodies, as, for example, lumps of charcoal soaked in water. Therefore the cause of the phenomenon of transpiration seems to be referrible entirely to physical agents. The *motion of the air* seems to be its exciting cause; for even when, to all appearance, it is calm, it is in reality agitated more or less, and produces a sensible evaporation from the skin. But the difference between the effect of calm and agitated air is remarkable; for in a draft, the animals exposed to it sweated away double the quantity of liquid compared with those confined in a room shut up. The amount lost was proportioned to the *intensity of the wind*, and reached a triple amount over those animals in stagnant air; and this fact explains the variations noticed from hour to hour among animals exposed to currents of air.

The transpiration which occurs in very moist air, always amounts to a diminution of weight; but in dry air it is five or ten times greater; and when the influence of a moist state of the atmosphere is compared with that of a dry state, the amount of evaporation is equal to that of a dry and calm air.

Transpiration may, therefore, be referred to *the agitation of the atmosphere* for its exciting cause, beyond any modifications of its *density*. And, although an elevated temperature be favourable to transpiration, its modifying influence is less than that of other causes.

In comparing the effects of *absorption* and *transpiration*,

in water and in air, frogs were found to gain an addition to their weight according to the term of their continuance in the former medium. An absorption of water was rendered evident by the loss of bulk it had sustained, when measured after the experiment.

Thus, when the comparative influence of water and air is estimated, the former appears to be absorbed, and adds to the weight of the body; and the latter tends to diminish the weight, by different and fluctuating degrees of evaporation taking place, and dependent much more on the degree of motion in the air, than on its dryness or humidity: these last conditions modify evaporation in a minor degree, when compared with the influence of a current of air.

The celerity of *absorption* exceeds that of *transpiration* six times, in the most rapid cases. It therefore results, that the losses by *transpiration* in air should be repaid by absorption of water in a much less time than the expenditure occurs. But the decrease of weight is not prolonged; it is sudden, and not continuous, alternating with augmentation of weight, by absorption of liquid going on in a ratio superior to the loss; and thus nature's provision is manifested for the nutriment of the body.

With this last inquiry Dr. Edwards concludes the first part of his work; and it is observed, that, with regard to transpiration, the losses of weight have been considered without reference to the existence of any other influence than water. The losses by transpiration have been examined generally without regard to the matters lost. What relates to water differs essentially in one respect from that which regards the air. The losses sustained by the body ought to be more particularly examined. Temperature and loss of time require estimation. An excretion of *solid* matter evidently takes place; for the water, in which animals are submersed, becomes turbid, especially in hot weather, and it sensibly contains animal matters, affecting the weight of the body in water.

When animals are submersed in water, their skins exercise two functions, acting inversely in determining their weight. And it results, from comparative experiments, that the *absorption at zero* exceeds the *loss in water;* while at thirty degrees the *loss* exceeds the increase by weight from absorption; and the higher the temperature, the greater is the excess in the discharge of animal matters. We may therefore presume, that the agency of *temperature* produces analogous effects, upon aërial transpiration, to those before observed

in other inquiries; and the effects of *dryness* and *moisture* in the air produce a minor degree of influence also, when compared with *temperature*, on the losses of animal substances.

We have been thus minute in our analysis, because the subject of it is new to science in its present shape, and of a high degree of interest. Dr. Edwards's researches among the different classes of animals have tended more to the illustration of the influence of physical agents upon life than any previous authorities; and the persevering industry, accuracy of observation, and patient inquiry which he has evinced in his investigations among cold-blooded animals, have placed this department of the creation in a point of view at once curious, interesting, and valuable to science. We attach the greater importance to this part of the author's work, as it is a ground on which he may be consulted, and quoted as indisputable authority, until equal inquiries have shewn him to be fallacious.

Our limits will not at present permit us to proceed farther in our analysis, and we must refer the remainder of the book to a future opportunity. The subjects of the three other parts, though greatly extended, will not probably require such minute analysis as those novel experiments which form the subject of the first part; but we imagine that the application of the principles laid down, in the previous inquiries, to human physiology, will be found not less interesting than those which relate to the natural history of the lower orders of the animal creation.

An Account of Professor Carlini's Pendulum Experiments on Mont Cenis.

We believe that no account of Professor Carlini's pendulum experiments on Mont Cenis has hitherto appeared in the periodical scientific publications of this country: the experiments are, however, well deserving of such notice, having been conducted with great care, and having had a specific object in view, which object seems to have been satisfactorily accomplished. The following brief account of them, taken from the original memoir published in the Appendix to the "Ephéméride di Milano" for 1824, may not be unacceptable to those of our readers who interest themselves in subjects of this class.

The length of the simple pendulum vibrating seconds is a

measure of the intensity of gravitation; *i. e.* of the excess of the force of gravity over the centrifugal force. In consequence of the ellipticity of the earth, and of the difference in the direction of the two forces, the intensity of gravitation varies according to the different latitudes. It also varies, in the same latitude, according to the greater or less elevation of the pendulum above the level of the sea; *i. e.* according to its greater or less distance from the centre of the attracting force.

Had the earth a perfectly level surface, such, for instance, as it would have if it were everywhere covered by a fluid, the force of gravity, in receding from the surface, would diminish in the duplicate proportion of the distance from the earth's centre. In the actual state of the globe, however, its continents and its islands are raised above the general level of the sea by which it is only partially covered; and if a pendulum be raised, on the surface of the land, to a known elevation above the sea, the diminution of gravity will not be, as in the more simple case, proportioned to the squares of the respective distances from the earth's centre, but that proportion will require to be modified, by taking into account the attraction of the elevated materials, interposed between the general surface and the place of observation.

When pendulums are employed in different latitudes, to obtain the ratio of gravitation between the equator and the pole, for the purpose of deducing the ellipticity of the earth, all the places of observation, being on land, are more or less elevated above the sea; inland stations, in particular, are sometimes at considerable elevations: to render these results comparable one with another, it is necessary to reduce each result to what it would have been, had it been made at some level common to all the experiments; and the surface of the sea has hitherto been taken as that common level. Previous to the publication of a paper of Dr. Young's in the Philosophical Transactions for 1819, the consideration which we have mentioned, that of the attraction of the matter interposed between the place of observation and the level of the sea, was generally unheeded in estimating the allowance to be made for the reduction of different heights to the common level: in that paper, however, Dr. Young took occasion to point out the probable effect of

the interposed matter in modifying considerably the usual allowance; that, supposing its density to be about half the mean density of the earth, the effect of an hemispherical hill of such matter, on the summit of which the pendulum should be placed, would be to diminish the correction, deduced from the duplicate proportion from the earth's centre, about $\frac{1}{5}$th; that, in like manner, a tract of table-land, considered as an extensive flat surface, of the same relative density, would diminish the correction about $\frac{3}{8}$ths; and that, accordingly, in almost any country that could be chosen for the experiment, the proper correction for the height would vary, according to the form and density of the interposed materials, from rather more than a half to rather less than three-quarters of the usual allowance. This view has been subsequently acted upon by the English pendulum experimentors, in reducing their observations; but it has not been yet adopted by the French. The experiments of Professor Carlini were calculated to afford a practical illustration of the correctness of Dr. Young's reasoning.

Professor Carlini was engaged, in the summer of 1821, in concert with Professor Plana, in determining the amplitude of the celestial arc between the Hospice on Mont Cenis and the Observatory at Milan, by means of fire-signals made on the Roche Melon, and observed simultaneously at Milan and at a temporary observatory established at the Hospice. Whilst thus engaged, Professor Carlini, being stationary for several days on Mont Cenis, and obliged to have time very accurately determined, for the purpose of comparing with the observatory at Milan, availed himself of the opportunity to employ a pendulum apparatus of the same general nature as that used by M. Biot at Paris, which had been prepared at Milan some years before, under the direction of a commission of weights and measures, with the view of determining the value of the divisions of the national linear scale. As this apparatus differed in some few particulars from the original employed in France, we shall briefly notice the differences, presuming our readers to be acquainted with the apparatus of MM. Borda and Biot.

1. In the Milan apparatus, by means of two microscopes furnished with wire micrometers, the length of the pendulum

may be measured without touching it; without approaching it; without even opening the case which contains it. The measure is obtained by bringing the wires in contact with the images of the knife-edge suspension, and of the upper and lower borders alternately of the platinum disk suspended to the thread: thus preventing the risk of deranging the equilibrium, and avoiding the effect which the heat of the body might have on the very dilatable metallic thread.

2. The half sum of the distances taken between the suspension, and the upper and lower edges of the disk, gives the distance of the centre of the disk itself, without measuring its diameter with a compass, an operation exceedingly difficult to execute with the necessary precision. By this apparatus of microscopes the length may be measured at pleasure, even during the time of oscillation; and being attached to the wall, instead of supported by the floor, the risk of derangement by the tread of the observer is avoided.

3. The pendulum, and the clock by which its oscillations are measured, were not, as usually, near together and resting on the same base, but were perfectly separated. The coincidences of the oscillations were observed, by bringing the image of the pendulum of the clock, reflected by means of an oblique mirror, in contact with the image of the simple pendulum seen direct through a telescope. By this modification the risk of the mutual influence of the pendulum and the clock is avoided.

4. The disk was attached to the thread by means of knots in the thread itself; avoiding the correction for the small cup usually employed for that purpose.

5. An alteration was made in the weight and shape of the knife-edge suspension; reducing its weight to about 10 grains, and giving it the shape of a rotella, instead of that of a triangular prism.

The simple pendulum and microscopes were attached to a strong wall, in a room on the ground floor, contiguous to the temporary observatory, and well sheltered from the sun and weather. The clock with which the pendulum was compared, was supported by a pyramid of masonry resting on the ground, and occupying the middle of the room. The experimental length between the microscopes was referred to three standard metres,

in perfect agreement with each other: one received from Paris by the Commission of Weights and Measures at Milan; a second brought more recently from Paris by Conte Moscati; and a third in the possession of the Royal Academy of Turin.

The experiments were commenced on the 3rd of September, and terminated on the 27th, being interrupted by M. Carlini's absence at Chambery from the 7th to the 12th. The distance between the microscopes, and the oscillations and length of the pendulum, were measured alternately. Thirteen independent results were thus obtained, of which the greatest discordance from the mean was not more than $\frac{13}{10000}$ths of a British inch. The mean result was 39.0992 British inches, the length of the pendulum vibrating seconds in a vacuum, at the place of observation on Mont Cenis, 1943 metres, or 6374 feet above the sea, in the latitude of 45° 14′ 10″. To compare with this determination, we may obtain a tolerably fair approximation to the pendulum at the level of the sea in the latitude of 45° 14′ 10″, such as its length might have been found, if the mountain could have been removed and the pendulum placed on its site, by deduction from the lengths actually measured with a similar apparatus, on the arc between Formentera and Dunkirk, at stations not far removed from the level of the sea, in the adjacent parallels to Mont Cenis, and in the countries adjoining. Of these there are five, not including the station at Clermont, in consequence of its great elevation: they are as follows:—

	°	′	″		
Dunkirk . .	51	02	10;	its pendulum at the level of the sea	= 39.13771
Paris . . .	48	50	14;	„ „ „ „	39.12894
Bordeaux .	44	50	26;	„ „ „ „	39.11295
Figeac . . .	44	36	45;	„ „ „ „	39.11212
Formentera	38	39	56;	„ „ „ „	39.09176

The mean length of the seconds pendulum at the level of the sea, in the latitude of 45° 14′ 10″, deduced from these determinations, is 39.1154; and it is so equally, whether an ellipticity of $\frac{1}{288}$th, or of $\frac{1}{304}$th, or any intermediate ellipticity, be assumed in the reduction.

We have, then, 39.1154 − 39.0992 = ·0162 inch., as the

measure of the difference in the intensity of gravitation at the place of observation elevated 1943 metres, and at the level of the sea. The radius of the earth being 6,376,478 metres, this measure, according to the duplicate proportion of the distances from the earth's centre, should be ·0238 inch. The attraction of the mountain is, then, equal to ·0238 − ·0162 = ·0076 inch. Whence it appears that, in this particular instance, the correction for the elevation is reduced, by the attraction of the interposed matter, to $\frac{68}{100}$ths, or to about $\frac{7}{10}$ths of the amount immediately deducible from the squares of the distances.

It is obvious that, if we possessed a correct knowledge of the density and arrangement of the materials of which Mont Cenis is composed, so as to enable a computation of the sum of all the attractions which they exercise on the place of observation, this result might furnish, as well as Dr. Maskelyne's experiments on the deviation of the plumb-line produced by the attraction of Mount Schehallien, a certain determination of the mean density of the earth. Professor Carlini considers that the form of the eminence may be sufficiently represented by a segment of a sphere, a geographical mile in height, having as its base a circle of 11 miles diameter, the distance from Susa to Lansleburgo; the attractive force, on a point placed on the summit, would, in such case, be equal to $2\,\pi\,\delta\,(1-\frac{2}{3}\sqrt{\frac{1}{11}})$, or in numbers to $5 \cdot 020\,\delta$, δ being the density of the mountain, and $2\,\pi$ the ratio of the circumference to radius. The attractive force of the earth, on a point at its surface, is $\frac{4}{3}\,\pi\,r\,\Delta$, $= 14394\,\Delta$, r being the radius of the earth $= 3437$ geographical miles, and Δ its mean density. Now these two quantities, $14394\,\Delta$ and $5 \cdot 020\,\delta$ should be, to each other, in the proportion of 39.1154,—the pendulum at the level of the sea, representing gravitation at the surface of the earth,—to ·0076, the portion of gravitation at the summit of the mountain due to the attraction of the mountain. By the observations of M. de Saussure and other geologists, Mont Cenis is chiefly composed of schistus, marble, and gypsum; the specific gravities of which substances were ascertained, from numerous specimens in the possession of M. Carlini, to be respectively as follows:—

The schistus . . .	2·81.
The marble . . .	2·86.
The gypsum . . .	2.32.

In the absence of a precise knowledge of the quantity and position of each of these three component parts, we may take the mean, 2.66, of their several densities as approximatively the density of the mountain, $= \delta$. We have then

$$\Delta = \frac{5.02\,\delta \times 39.1154}{14394 \times \cdot 0076} = 4.77,$$

a result differing little from that of Cavendish as recently corrected by Dr. Hutton, and still less from that of the Schehallien experiments.

The most hypothetical element of this calculation is the width assigned to the base of the mountain; but by the very nature of the question, it has but little influence on the final result; since, by even doubling the assigned diameter, the total attraction would not be altered a twentieth. In regard to the mean density of the mountain, if it were taken at 2.75, instead of 2.66, that of the earth would result 4.94, instead of 4.77, as given above.

E. S.

Transactions of the Horticultural Society. Vol. vii. Part 1. 4to. London, 1827. pp. 208.

I. *Observations upon the Growth of Early and Late Grapes under Glass.* By Mr. James Acon.

Few gardens are to be found in which bunches of fresh ripe grapes can be gathered every day in the year: notwithstanding the importance of the fruit to the luxurious, and the facility with which the vine submits to the artificial climate of the forcing-house. Nothing is easier than to secure crops of grapes in a vinery during the spring and summer months; but it is far more difficult to obtain them in the last and earliest seasons of the year, when the plants would

naturally be in a state of torpidity. It is well known that this desirable purpose is attained in great perfection in the garden of the Earl of Surrey, at Worksop Manor; and the management there practised is the subject of this paper.

The common methods of forcing early grapes are to train the vines under the roof near the glass, or on small frames against flued walls; but to both these practices Mr. Acon finds great objections: to the former because it renders the house too dark, and exposes the young and tender branches to the pernicious effect of blasts of cold air rushing through the interstices of the panes; and to the latter, because the heat of the flues is apt to scorch the branches, and in consequence to destroy the crop,—excessive heat in the one case producing the same injurious effects as excessive cold in the other. The following are the two modes by which Mr. Acon obtains his *very early* and his *very late* grapes. For the early crops a house is used, of which the back wall is 9.6 feet in height, and the front wall 3 feet, the roof forming an angle of about 30 degrees. It is heated, from the absolute necessity of employing an atmosphere of unusually high temperature, with two flues that pass along the middle of the house, and return in the back wall; a fire-place being built at each end of the house. Forcing begins on the first of September, and the fruit begins to ripen the first week in March. The vines are trained upon a trellis, fixed over the flues, in the centre of the house, and also upon the back wall; but none are allowed to obstruct the light by occupying the roof, until about six weeks after the forcing has commenced, when some new shoots are introduced and trained to the rafters. The form of this house gives it a peculiar advantage, in presenting a greater surface for the growth of vines than can be derived from any other plan; the trellis which is placed over the flues is nearly equal to the whole roof, without being in any degree injurious to the plants trained upon the back wall. The vines are planted in the inside of the house, but in such a manner that the mould in which they grow is not heated by the fire-places of either flue. The usual mode of exposing the main stem of a forced vine to an extremely low temperature in the external air, while the branches are stimulated by a very high temperature in an entirely different atmosphere, is very properly objected to. Nothing, in fact, can be more injudicious than such a practice, in cases where very early forcing is required; for it should be borne in mind, that although the absorption of the elements by which the proper juices of a

plant are elaborated, and brought into the state under which they appear in the fruit, and in the secretions of the plant, is carried on by the leaves alone, yet that all these juices have, in the first instance, to pass along the vessels of the stem before they reach the leaves; and that the whole of the bark of a tree is, rightly considered, a leaf of a particular description, formed of the same kind of tissue, and exercising the same functions, and undoubtedly producing a powerful effect upon the motion of the fluids of the branches, with the vessels of which it is elaborately and intimately entangled, from the core to the circumference. No argument can be necessary to show that an equal action of the vessels of a plant is indispensable to the due maintenance of the vegetable functions in a healthy state, and that this is not to be maintained by exposing the main stem and the extremities to an atmosphere and temperature entirely different. Such irregularities do not exist in free Nature, and she will not submit to them when in fetters.

In pruning vines for early forcing, as little wood should be employed as possible. Mr. Acon stops the shoots one joint above each cluster, and has no joint without a bunch. When the crop is over, and the wood perfectly matured, the branches should be laid near the ground, and shaded till the recommencement of forcing. In short, they should be placed in a condition as nearly as possible resembling the gloom and cold of winter. If this process be well managed, the vines will alter their natural habits, and instead of budding with the spring, their vegetation will naturally commence at the period at which they have been accustomed to be stimulated.

For late grapes, a house of a different construction is employed. The back wall is 12 feet high, the front wall 1½ foot, and the roof lies at an angle of 45 degrees. The heat is supplied by a single flue passing along the middle of the house. The sorts best adapted for late forcing are the Muscat of Alexandria, the St. Peter's, and the Black Damascus; all other kinds wither prematurely. This house is generally shut about the middle or end of May, as soon as the bunches become visible. The vines are trained on a trellis near the glass. Till they are out of blossom the air is kept very warm, a point to which much importance attaches, because it is during this period that all the branches that are to bear fruit in the succeeding season are produced. In a high temperature, the branches will grow more compactly, and

will be more regularly matured than in a low temperature, in which the wood is apt to become excessively luxuriant, and not to ripen well. Great attention must be paid to this point. As much air as possible is introduced into the vinery during the summer; but as the autumn advances, more caution in this respect is observed. The fruit should be perfectly coloured at the approach of the dark season; for if the colouring be deferred too long, the berries will never acquire their proper flavour. Great care must be observed to remove daily such berries as are inclining to damp, or the whole crop will soon be spoiled. This should be particularly attended to; for the contagion of what gardeners call *damp*, arises from the growth of minute fungi which vegetate upon the epidermis, and spread during the autumn with alarming rapidity from bunch to bunch.

The pruning of vines for late forcing is the same as has been already explained. When the crop is gathered, the house is unroofed for a short time, in order to expose the branches to a low temperature, and to the degree of humidity necessary to replenish their vessels, which have been drained by the dryness of the climate in which, when forced, they were necessarily kept.

By the means above described, a regular supply of grapes is secured through the year. The late-house crop lasts from the middle of January to the end of March; it is succeeded by the first crop in the early-house, which carries on the supply into May, and it is continued by the grapes on the rafters in the same house until the vines in the pine stoves, which are forced early in January and February, produce their crops. These continue bearing through the summer, when a vinery, of which the forcing commences about the end of March, furnishes the supply till the late-house fruit is ready in January.

Upon the whole this may be considered a most instructive and valuable communication.

II. *On the Varieties of Cardoon, and the Methods of cultivating them.* By Mr. A. Mathews.

Who does not wish to read of the cardoon; of that prince of vegetables, whose praises have been sung or said by all cooks and gourmands, from the fastidious Périgords and Cardellis of the French *cuisine*, down to the more homely Rundells and Glasses of our English kitchens; whose virtues are so marvellous as to be credible upon no less authority

than that of the sage gastrophilists aforesaid. To restore unwonted vigour to old age, and new elasticity to youth, are the most modest of its attributes; the magical broth with which the veins of Æson were replenished by the cunning Medea, was doubtless prepared from the cardoon; and the story itself is probably a sort of figurative record of the skill of the fair enchantress in cooking this delicious vegetable, which was well known to the Grecian gastronomes under the name of κακτος; but this we throw out merely as a suggestion. Upon preparing herbs thus potent for the table, cookery has exhausted all its skill; to dress a cardoon is declared, by the highest authority in the art, to be the surest test of a skilful cook; and one of those invaluable acquirements which, to borrow the words of a writer not less celebrated for his powers of composition than of cooking, "raises cookery to the rank of the *sciences*, and its *professors* to the title of artists." Our good forefathers, indeed, "could not find the true manner of dressing cardoons," and were content to eat them raw "with vinegar and oyl, pepper and salt, all of them, or some, as every one liketh for their delight;" which, considering that this vegetable is both bitter and astringent in a high degree, does not argue much for the delicacy of palate of our ancestors; little did they dream of the savoury preparations that modern art has devised by the aid of Espagnole, consommé, blancs, tammies, marking, masking, and all the mysteries of the stewpan.

Four varieties are here described, of which the Spanish cardoon is the most common, and the cardon de Tours the best.

They are cultivated, like celery, in deep broad trenches, well manured and watered. When the plants are nearly full-grown, which will be about the end of October, a dry day is to be chosen for performing the operation of blanching them, which is thus effected:—

"The leaves of each plant are carefully and lightly tied together with strong matting, keeping the whole upright, and the ribs of the leaves together. The plant is then bound closely round with twisted haybands, about an inch and a half in diameter, beginning at the root, and continuing to about two-thirds of its height. If the plants are intended for winter store, they must be earthed up like celery; but if to be consumed before the frosts set in, the operation of earthing up may be omitted."

III. *Accounts and Descriptions of the several Plants belonging to the genus Hoya, which are cultivated in the garden of the Horticultural Society at Chiswick.* By Mr. James Traill.

The beauty of one species of Hoya, viz., *H. carnosa*, has long caused it to be a favourite with collectors. The object of the writer of this paper is to call attention to such others as are known to exist in gardens, or as are preserved in the records of the botanist.

The following species form the subject of the paper, viz.:

1 Hoya carnosa, *R. Brown*. 2 Hoya crassifolia, *Haworth*. 3 Hoya pallida, *Lindley*. 4 Hoya Pottsii, (Tab. I.) 5 Hoya trinervis.

These five are all the species at present cultivated in gardens; others are known to exist in the warmer regions of Asia, where they should be assiduously sought for by travellers, as they are not only very ornamental, but also easily to be transported to Europe.

From such materials as he has been able to procure, the writer enumerates the following as completing the genus Hoya, as far as at present ascertained:

6 Hoya chinensis. 7 Hoya viridiflora, *R. Brown*. 8 Hoya lanceolata, *D. Don*. 9 Hoya linearis, *D. Don*. 10 Hoya australis, *R. Brown, MSS.* 11 Hoya nicobarica, *R. Brown, MSS.* 12 Hoya augustifolia.

The paper concludes with a detailed explanation of the best manner of cultivating Hoyas.

IV. *On acclimatizing Plants at Biel, in East Lothian.* By Mr. John Street, gardener to the Honourable Mrs. Hamilton Nesbitt.

Perhaps there is no point whatever, connected with Horticulture, of greater interest than that which forms the subject of this paper; it is the distant goal towards which we all are striving, but of which, alas! we have not as yet even caught a glimpse. The gardener is in possession of the powers by which he can bend the seasons to his will; he can dispel the frozen gloom of winter with the rich warm glow of the vintage; at his call the flowers of spring and summer start up beneath his feet, and his hothouses are filled with the luscious fruits of the torrid zone. All this he knows how to effect with an artificial climate; but he has no influence over the natural climate of his country, nor can he impart to the vegetation of warmer latitudes the least additional power of resisting cold, for which they have not been prepared by nature. Acclimatizing is still a secret to be discovered. To

this day not a single instance can be adduced of any exotic plant whatever possessing greater powers of withstanding cold, than it had when first introduced. It has been hoped that if the seeds of a given plant could be procured, for many generations, in a climate severer than its own, the offspring so obtained would gradually accommodate themselves to their new country; but no such result has followed from the experiments that have been tried. Let us take a few familiar examples:—the common nasturtium, (Tropæolum majus,) a native of Peru, is said to have been introduced about the year 1686. At the time at which we are writing, it must have descended through about 140 generations; and yet it has not become in the smallest degree capable of resisting cold. Of the mignonette (Reseda odorata), the date of introduction is not well ascertained; it has probably been a favourite border annual for sixty or seventy years, and yet it has in no degree shaken off its annual character, which is unnatural to it, and resumed the suffrutescent habit which it possesses in its own milder climate. The potato, too, which has for two centuries and a half been increased in every conceivable manner, by seeds as well as by offsets, bears cold in no degree more readily than it did in the sixteenth century. Nor does it appear to us probable, that acclimatizing, if practicable, is to be brought about by sowing seeds in northern latitudes through successive generations. We do not believe that plants will bear their seeds at all in a temperature much lower than that in which they have been located by the hand of Nature. The heat of a northern summer sufficiently approximates to that of the tropics, to be considered, with reference to vegetation, as the same, and it is during that season that the seeds of all plants are ripened; the conditions, therefore, under which the seeds of Tropæolum, for example, are produced in England, do not materially differ from those under which the same seeds are produced in Peru; if the season proves unpropitious in any considerable degree, they are not produced at all. How then can it be expected that seeds ripened under similar circumstances, but in different latitudes, should give birth to a progeny differing in any remarkable particular from their parents? In fact, in power of resisting cold, they do not differ at all. If such a capability were to be obtained, it would be by inducing plants to ripen their seeds in winter.

But if it is certain that nothing is to be gained in acclimatizing, by raising plants from seed through successive gene-

rations, it is no less true that many trees, which have been supposed to be incapable of surviving a northern winter, are now ascertained to be perfectly hardy, and that the power of enduring cold may be increased in others, by a judicious management of soil and situation.

The phenomenon of vegetable life being destroyed by cold, probably arises from the vessels, through which the circulation and secretion of the fluids of plants take place, being ruptured by the expansion, from cold, of the fluid they contain. In proportion, therefore, to the tenuity of the vessels, and the abundance of their fluid, will be the danger to which they are exposed from frost; and to the strength of the vessels, and the paucity of their fluid, the power of resisting cold. Thus vigorous shoots of the oak, walnut, and many other trees, which are formed with rapidity, imperfectly matured, and highly charged with fluid, are extremely impatient of cold, and are even destroyed by a few degrees of frost; while the twigs and branches of the same trees, which are formed slowly, fully matured, and incompletely filled with fluid, bear unharmed the utmost rigour of our winters. In acclimatizing, therefore, this law should be carefully remembered, and the situations in which tender plants are stationed, should be those in which their growth is restrained, and an excessive absorption of fluid prevented.

This appears to have been the true secret of the success that has attended the attempts at acclimatizing, which form the subject of Mr. Street's communication. By planting in situations well drained from superfluous moisture, under circumstances where rapid growth was rendered impracticable, and, as we understand, in a garden admirably adapted to the object, from its position, he has succeeded in naturalizing, in latitude 56° N., plants which have not yet been known to endure the winters even of the parallel of London.

V. *Upon the Culture of Celery.* By Thomas Andrew Knight, Esq., F.R.S., President.

"That which can be very easily done, without the exertion of much skill or ingenuity, is," Mr. Knight observes, "very rarely found to be well done, the excitement to excellence being in such cases necessarily very feeble." This remark is in the present case applied to the cultivation of celery, which, being a native of the sides of wet ditches, might naturally be expected to demand an abundant supply of water when cultivated. Accordingly, Mr. Knight found that by keeping the ground, in which celery was planted, con-

stantly wet, it grew by the middle of September to the height of five feet, and its quality was in proportion to its size. Mr. Knight also recommends planting at greater distances than is usually the case, and covering the beds, into which the young seedlings are first removed, with half-rotten dung, overspread to the depth of about two inches with mould; under which circumstances, whenever the plants are removed, the dung will adhere tenaciously to their roots, and it will not be necessary to deprive the plants of any part of their leaves.

VI. *Report upon the New or Rare Plants which flowered in the Garden of the Horticultural Society at Chiswick, between March,* 1825, *and March,* 1826. Part I. *Tender Plants.* By John Lindley, Esq.

The subject of this paper consisting of botanical details which do not bear curtailing, we shall only extract the names of the new species described in it, as a guide to our botanical readers. In the whole, thirty-three species are noticed; of which the following are published for the first time:—

2 Passiflora obscura. 7 Solanum dealbatum. 10 Tabernæmontana gratissima. 13 Tephrosia? Chinensis. 15 Hellenia abnormis. 16 Gesneria Douglassii. 21 Gynandropsis pulchella. 23 Rodriguezia planifolia. 26 Brassavola nodosa. 33 Phycella corusca.

VII. *Account of a Protecting Frame for Fruit-Trees on Walls.* By Mr. John Dick.

In order to protect the fruit upon walls from the ravages of bees, wasps, flies, and other winged enemies, a frame is contrived fitting close to the face of the wall, and having a moveable sliding canvass front, which can be readily removed when the fruit is to be gathered, and replaced again afterwards. A plan of the frame accompanies the paper. From what we have seen of this contrivance, we know that it is well adapted to its purpose, and that no garden in which fine fruit is required, should be without one or more of such frames. For the mode of making them, we must refer to the paper itself.

VIII. *On the Esculent Egg-Plants.* By Mr. Andrew Mathews.

In this country, the egg-plant, brinjal, or aubergine, is chiefly cultivated as a curiosity; but in warmer climates, where its growth is attended with less trouble, it is a favourite article of the kitchen-garden. In the form of fritters, or farces, or in soups, it is frequently brought to table in all the southern parts of Europe; and forms a pleasant va-

riety of esculent. This paper describes the only two kinds that are worth cultivation in England.

IX. *Notices of Communications to the Horticultural Society, between January* 1, 1824, *and January* 1, 1825. *Extracted from the Minute Books and Papers of the Society.*

A novel kind of pine pit is described, which is said to answer every purpose that can be desired. It is heated by flues passing through a chamber, formed by beams extending from the back to the front wall, and so becoming a sort of floor, upon which is first placed a layer of turf; and then the tan in which the pine-plants are plunged. The warmer air is conveyed into the upper part of the pit by means of small apertures contrived in the walls, at four inches and a half apart, both in the back and front of the pit, and also through iron pipes resting on the beams and passing through the tan. The ventilation is effected by air-holes in the front wall, and sliding shutters in the back walls. An explanatory figure accompanies the statement.

The famous rhubarb, which has of late acquired so much celebrity under the name of Buck's rhubarb, is mentioned as excellent when forced. It is not generally known, that this sort is the genuine Rheum undulatum of botanists uncontaminated by mixture with the common garden kinds. The plant generally called Rheum undulatum, is a half-bred, possessing none of the good qualities of the native species.

George Tollet, Esq., of Betley Hall, in Staffordshire, recommends the preservation of apples for winter store, packed in banks or hods of earth like potatoes. The method is said to be effectual and economical.

Thomas Bond, Esq., of East Looe, in Cornwall, describes his mode of cultivating strawberries. He does not adopt the common practice of cutting off the runners, but they are confined to the bed by being turned back among the plants from which they spring. In the autumn, the beds are covered to the depth of two inches with fresh earth, through which the strawberry-plants shoot in the spring with great vigour.

A kind of wicker basket is described, which is cheap and well adapted for screening half hardy plants during the winter. It is fixed in the earth by means of the points of the ribs of the wicker work, which are allowed to project a few inches for the purpose.

It is stated by John Wedgewood, Esq., that good celery may be readily obtained by transplanting seedling plants that have remained in the seed bed, till they had acquired a

considerable size. They grow more vigorously than the younger plants that are transplanted in the usual way.

William Cotton, Esq., of Wellwood-house, describes the good effects of painting an old garden wall with seal oil and anticorrosion paint. The wall in question was covered with trees, which were every year attacked by blight. Since the operation the trees have borne good fruit, made healthy wood, and been free from the bad consequences of blight.

Mr. John Mearns states, that the red and white Antwerp raspberries may be brought to bear abundantly in August, long after the usual crop of raspberries is past, by the following management. In May he removes the young fruit, bearing shoots, from the canes, leaving in some cases one or two eyes, in others, cutting them clean off. Under either plan, they soon produce an abundance of vigorous new shoots, which blossom freely in July.

Mr. Elias Hildyard, gardener to Sir Thomas Frankland, kills the grub which infests his onion beds by trenching the beds in winter, digging in manure at the same time, and leaving them exposed to the frost in a rough state till the time of sowing.

A mode of inducing fertility in a barren Swan's-egg pear-tree trained upon a wall, is described by the Rev. John Fisher, of Wavenden, in Buckinghamshire. It consists in twisting and breaking down the side shoots of the main branches in such a way, as to make them pendulous without separating them wholly from the parent limb. In a short time a grumous formation takes place where the fracture has occurred, the wound heals, the flow of the sap is moderated, and fruit buds are formed instead of sterile shoots.

Mr. William Mowbray, gardener to the Earl of Mountnorris, states, that the different species of eatable Passifloras which do not generally produce fruit, may be induced to do so abundantly, if the pollen of other species is applied to their stigmas.

Currants are preserved in perfection in the garden of James Webster, Esq., of Westham, by being covered with bunting when the fruit is fully ripe, care being had to unloose the bunting occasionally from the bottom of the bushes, in order to remove the decaying fallen leaves.

X. *Report on the Instruments employed in, and on the Plan of a Journal of Meteorological Observations, kept in the garden of the Horticultural Society at Chiswick.*

This and the following paper we propose to notice in detail on a future occasion.

XI. *Journal of Meteorological Observations made in the garden of the Horticultural Society at Chiswick, during the year* 1826. By Mr. William Beattie Booth.

XII. *On Orache, its Varieties and Cultivation.* By Mr. William Townshend.

The herb orache was formerly cultivated as a kind of summer spinach; but in this country it has long been expelled from the kitchen garden by other kinds. It is, however, still seen in the gardens of France, where it is commonly called Arroche des jardins, being used in that country, both by itself as a spinach, and mixed with sorrel, the acidity of which it corrects. Seven varieties are described, which do not differ in their qualities, but are distinguished by the colour of their foliage.

XIII. *On planting the moist Alluvial Banks of Rivers with Fruit-Trees.* By Mr. John Robertson.

The object of this writer is to show that the low grounds that form the banks of rivers are, of all others, the best adapted for the growth of fruit trees; the alluvial soil of which they are composed, being an intermixture of the richest and most soluble parts of the neighbouring lands, with a portion of animal and vegetable matter, affording an inexhaustible fund of nourishment. In such situations, however, the trees are liable to injury from floods in the winter, unless some means are used of draining off the stagnant water. This is to be effected by digging deep trenches between the rows of trees, casting up the earth from the trenches around the trees on either side, so as to form elevated banks. Such is the practice in Holland, where the western slopes of the dykes are generally covered with fruit-trees, chiefly apples and pears. Mr. Robertson is of opinion, that the banks should be raised, if possible, at least three or four feet above the highest water-mark, and be made eighteen feet broad at the base, and twelve at top; the trenches should be fifteen or sixteen feet wide, admitting the soil to be three or four feet deep.

Upon this plan, it is probable that abundant crops would be obtained; but with regard to the quality of the produce, we suspect it will be quite as indifferent as the apples and pears of the Dutch, which are notorious for their want of flavour.

XIV. *On Dahlias.* By Mr. William Smith.

This is an attempt to distinguish by words the best varieties of the Dahlia, and to fix the names of those which are the most worthy of cultivation. Sixty kinds are well described,

arranged in divisions depending upon the size of the plants and the colour of their flowers. We do not propose to analyze this paper, which is far too extensive for our limits; but instead, to throw together a few remarks which are suggested by the subject.

The first fact to which we would call attention has reference to acclimatization. The Dahlia has now been cultivated in Europe with the utmost assiduity for nearly thirty years. During that period millions of plants have been raised from seeds, and under almost every possible variation of climate; and anomalies the most singular, not only in colour, but in general constitution and physiological structure, have been obtained. The colour of the flower has been altered from pale yellow, or lilac, to every hue of red, purple, or yellow, to pure scarlet and to deepest morone, or has even been wholly discharged from the radial florets in the white varieties; the period of flowering has been accelerated nearly two months; the tall rank weed, exceeding the human standard in height, has been reduced to a trim bush, emulating the pæony in dwarfishness; the yellow inconspicuous florets of the disk have been expelled to make room for the showy deep-coloured florets of the ray; what is more remarkable still, the same yellow inconspicuous florets of the disk have been enlarged, and stained with rich morone, so as to rival the colours of the ray without losing their own peculiarity of form; and finally, the whole foliage and bearing of the plant has been altered by the substitution of simple leaves for compound ones. But notwithstanding all this proneness to change, notwithstanding the multitude of varieties which have been thus procured by seed, *not one individual has yet been discovered, in any degree whatever, more hardy than its ancestors.* The earliest frosts destroyed the Dahlias as certainly in 1826, as they could have done in 1789.

But, however strong may be the disposition of the Dahlia to vary from its original structure, it is curious to observe how strictly it conforms to the laws by which such variations are controlled by nature. In altered structure all the changes take place from circumference to centre. The florets of the ray displace those of the disk, but the latter never attempt to occupy the ray; when a change occurs among the florets of the disk, they merely dilate and assume the colour of the ray, without changing their position or their peculiar form. So with the leaves; by a reduction of the lateral leaflet, till the terminal one only remains, simple foliage is substituted for that which was compound; but no case has been found in

which the suppression of the terminal leaflet has taken place and the lateral ones have been preserved. In change of colour, too, there is a circumstance which demands consideration, and of which no explanation has yet been offered. It is not generally known, although long ago noticed by M. De Candolle, that among flowers, yellows will not produce blues, nor blues yellows, although both these primitive colours will sport into almost every other hue. Thus the hyacinth, the natural colour of which is blue, will not produce a yellow, for the dull, half-green flowers called yellow hyacinths, are, in our judgment, whites approaching green; the blue crocus will not vary into yellow, nor the yellow into blue; and the ranunculus and the dahlia, the natural colour of both which, notwithstanding the popular belief to the contrary, with respect to the dahlia, is, we believe, yellow, although they are the most sportive of all the flowers of the gardens, varying from pink to scarlet, and deepest shades of purple, have never yet been seen to exhibit any disposition to become blue. This subject offers a most amusing field for investigation, and would well repay the attentive consideration of the philosopher.

XV. *On the Cultivation of Camellias in an open Border.* By Mr. Joseph Harrison.

Mr. H. finds that the double red camellia, the double white, and the double striped, will bear an English winter if planted out when about two feet high, having been previously stunted in their growth by repeatedly stopping their leading shoots. For two winters the young plants are to be protected by a wooden screen fixed round them, and covered by a hand-glass, the whole being enveloped in mats; afterwards they require no other protection than to be guarded from heavy snow-storms, and to be assisted by a thick covering of old tan upon the ground in which they grow, to the distance of two or three feet from their stems. If this success has been met with in Yorkshire, what may not be expected in our more southern counties! On the 12th of March of the present year, these camellias were not injured by a frost which did considerable damage to the common laurel.

XVI. *A Method of growing Crops of Melons on open Borders.* By Mr. William Greenshields.

The sorts fitted for this purpose are the black rock, scarlet rock, green-fleshed, netted and early Cantaloup melons. The method consists of forming a bed, by half filling a shallow

trench with decayed vegetables, and covering them with the exhausted linings of cucumber beds. The young plants are reared for some time under handlights. For full particulars of this practice, we must refer to the paper itself, which is clearly written, and, coming as it does from one of our most skilful gardeners, well worthy of attention.

XVII. *Notice of Five Varieties of Pears received from Jersey in the year* 1826. By John Lindley, Esq.

The fruits here described are of the highest excellence. They are, 1. the Marie Louise; 2. the Duchesse d'Angoulême; 3. the Doyenné gris; 4. the Doyenné panaché; 5. the Beurré d'Aremberg; and 6. the Gloux morceaux. The second, the fifth, and the sixth kinds are represented in two very beautiful coloured plates; and are, perhaps, the most exquisitely flavoured of all the varieties of the pear. The Beurré d'Aremberg and Gloux morceaux are long keepers; the others are autumnal kinds. Of the former it is said, "the flesh is whitish, firm, very juicy, dissolves in the mouth, and is wholly destitute of grittiness; it is sweet, rich, and so peculiarly high flavoured, that I know no pear that can be compared with it in that respect."

XVIII. *Upon the Culture of the Prunus Pseudo-cerasus, or Chinese Cherry.* By Thomas Andrew Knight, Esq.

This species of cherry is expected to become an acquisition of considerable value, for the purpose of forcing; and also as an early fruit, when trained upon an open wall. Mr. Knight recommends its propagation by cuttings, which root freely, and that it be abundantly supplied with liquid manure. From its highly excitable habits, he suspects it to be a native of a cold climate, probably of Tartary.

XIX. *On the Culture of the Pine-Apple.* By Mr. James Dall.

XX. *On forcing Asparagus.* By the same.

These two papers were communicated by the Cambridge Horticultural Society, having gained one of the annual silver medals presented by the London to Provincial Societies. They contain good practical directions for the cultivation upon which they treat.

XXI. *Observations upon forcing Garden Rhubarb.* By Mr. William Stothard.

This plan is perhaps the best that can be followed, as it is at once the most certain and the most simple. You sow rhubarb seed on a rich moist border in the beginning of April,

The young plants are well thinned during the summer; in the end of October they are very carefully transplanted into forcing-pots, five or six in each pot. They are placed in a north aspect, to recover the effect of their removal from the seed-bed, and in a month they are fit for forcing. We can safely recommend this method.

XXII. *Account of some remarkable Holly Hedges and Trees in Scotland.* By Joseph Sabine, Esq.

This is an elaborate account of some extraordinary specimens of hollies, and appears to have been written with a view to induce the more general cultivation in this country of that very valuable tree. At Tynningham, the residence of the Earl of Harrington, are hedges extending to no less a distance than 2952 yards, in some cases thirteen feet broad, and twenty-five feet high. The age of these hedges is something more than a century. At the same place are individual trees of a size quite unknown in these southern districts. One tree measured five feet three inches in circumference at three feet from the ground; the stem is clear of branches to the height of fourteen feet, and the total height of the tree is fifty-four feet. The other places at which the hollies are of unusual size, are Colinton-house, the seat of Sir William Forbes; Moredun, the seat of David Anderson, Esq.; Hopetoun-house, the seat of the Earl of Hopetoun, and Gordon-castle, where are several large groups of hollies, apparently planted by the hand of Nature.

XXIII. *An Account of a Plan of Heating Stoves by means of Hot Water, employed in the Garden of* Anthony Bacon, Esq.

We conceive that a new æra in horticulture will commence with the publication of this paper. We already possessed contrivances of a sufficiently good kind for all purposes connected with artificial climate, except the power of commanding heat; for which the two methods hitherto employed have been either too clumsy or too costly, and in either case liable to numerous objections. The old mode of introducing heat into a stove, by means of brick flues, has long been considered so bad, that every scheme that promised to supersede such flues has been hailed with joy; the uncertainty of the quantity of heat given out by a brick flue, its continual liability to explosion, the impossibility of preventing the escape of smoke from between the joints of the bricks, are all evils that require a remedy. For this purpose steam was introduced, and with great advantage in extensive ranges of hothouses. But the enormous expense of erecting a steam

apparatus, the danger attending its use in the charge of an unskilful or careless gardener, and also the rapid loss of heat from the pipes upon any neglect of the boiler, have all contributed to prevent the use of steam becoming very general. The plan now described has the great merit of possessing all the good qualities of steam, without any of its objectionable accompaniments; its cost cannot in any considerable degree exceed that of flues, and its effects are so certain and durable, that a house so heated may be almost said to be beyond the power of neglect on the part of the gardener.

Without entering into the details of this plan, for which we must refer to the paper itself, we shall content ourselves with explaining its principle. Suppose two iron reservoirs, A and B, of equal capacity, placed twenty feet apart, and connected at the top and the bottom by iron pipes, the level of both reservoirs being the same; it is obvious that water poured into one of these reservoirs will flow into the other through the connecting pipes, and that it will consequently stand at the same height in both. Let the reservoirs be thus filled above the level of the uppermost pipe, and heat be applied to the bottom of one reservoir, A; the water in this will presently be forced through the upper pipe into the reservoir, B, of water not heated; in proportion as the heated water flows out of A, through the upper pipe, the cold water will flow out of B through the lower pipe; and by this means a circulation of water heated and water to be heated will be formed, which will continue as long as the application of fire to the bottom of one reservoir is continued. When it is discontinued, the temperature of the two reservoirs and of the intermediate pipes will be the same within three or four degrees. As it is the property of heated water to part with its heat very slowly, it follows that heat will continue to be disengaged from the reservoirs and pipes long after the application of fire has ceased. In fact, when the two reservoirs are once heated, the gardener may make up his fires and retire to rest, certain that his house is sufficiently provided with heat for the night.

The paper is accompanied with a plan of a vinery warmed upon this principle.

On the Recent Elucidations of early Egyptian History.

Since the commencement of the present century, the researches of philologists have ascertained that the language of ancient Egypt,—the language of the hieroglyphical inscriptions engraven on its ancient temples and monuments, and of the still existing manuscripts of the same period,—differs from the modern Egyptian or Coptic, only in the mixture in the latter of many Greek and Arabian and a smaller portion of Latin words, introduced during the successive dominion of the Greeks, the Romans, and the Arabs, and occasionally substituted for the corresponding native words. The grammatical construction of the language has remained the same at all periods of its employment: and it finally ceased to be a spoken language towards the middle of the seventeenth century, when it was replaced by the Arabian.

In writing their language, the ancient Egyptians employed three different kinds of characters. First, *figurative;* or representations of the objects themselves. Second, *symbolic;* or representations of certain physical or material objects, expressing metaphorically, or conventionally, certain ideas; such as, a people obedient to their king, figured, metaphorically, by a bee; the universe, conventionally, by a beetle. Third, *phonetic,* or representative of sounds; that is to say, strictly alphabetical characters. The phonetic signs were also portraits of physical and material objects; and each stood for the initial sound of the word in the Egyptian language which expressed the object pourtrayed: thus a lion was the sound L, because a lion was called Labo; and a hand a T, because a hand was called Tot. The form in which these objects were presented, when employed as phonetic characters, was conventional, and *definite* to distinguish them from the same objects used either figuratively or symbolically; thus, the conventional form of the phonetic T was the hand open and outstretched; in any other form the hand would either be a figurative, or a symbolic sign. The number of distinct characters employed as phonetic signs appears to have been about 120; consequently many were homophones, or having the same signification. The three kinds of characters were used indiscriminately in the same writing,

and occasionally in the composition of the same word. The formal Egyptian writing, therefore, such as we see it still existing on the monuments of the country, was a series of portraits of physical and material objects, of which a small proportion had a symbolic meaning, a still smaller proportion a figurative meaning, but the great body were phonetic or alphabetical signs: and to these portraits, sculptured or painted with sufficient fidelity to leave no doubt of the object represented, the name of hieroglyphics, or sacred characters, has been attached from their earliest historic notice.

The manuscripts of the same ancient period make us acquainted with two other forms of writing practised by the ancient Egyptians, both apparently distinct from the hieroglyphic, but which, on careful examination, are found to be its immediate derivatives; every hieroglyphic having its corresponding sign in the *hieratic*, or writing of the priests, in which the funeral rituals, forming a large portion of the manuscripts, are principally composed; and in the *demotic*, called also the *enchorial*, which was employed for all more ordinary and popular usages. The characters of the hieratic are for the most part obvious running imitations, or abridgments of the corresponding hieroglyphics; but in the demotic, which is still further removed from the original type, the derivation is less frequently and less obviously traceable. In the hieratic, fewer figurative or symbolic signs are employed than in the hieroglyphic; their absence being supplied by means of the phonetic or alphabetical characters, the words being spelt instead of figured; and this is still more the case in the demotic, which is, in consequence, almost entirely alphabetical.

After the conversion of the Egyptians to Christianity, the ancient mode of writing their language fell into disuse; and an alphabet was adopted in substitution, consisting of the twenty-five Greek letters, with six additional signs expressing articulations and aspirations unknown to the Greeks, the characters for which were retained from the demotic. This is the Coptic alphabet, in which the Egyptian appears as a written language in the Coptic books and manuscripts preserved in our libraries; and in which, consequently, the language of the inscriptions on the monuments may be studied.

The original mode in which the language was written having thus fallen into disuse, it happened, at length, that the signification of the characters, and even the nature of the system of writing which they formed, became entirely lost; such notices on the subject as existed in the early historians being either too imperfect, or appearing too vague, to furnish a clue, although frequently and carefully studied for the purpose. The repossession of this knowledge will form, in literary history, one of the most remarkable distinctions, if not the principal, of the age in which we live. It is due primarily to the discovery by the French, during their possession of Egypt, of the since well-known monument called the Rosetta Stone, which, on their defeat and expulsion by the British troops, remained in the hands of the victors, was conveyed to England, and deposited in the British Museum. On this monument the same inscription is repeated in the Greek and in the Egyptian language, being written in the latter both in hieroglyphics and in the demotic or enchorial character. The words Ptolemy and Cleopatra, written in hieroglyphics, and recognized by means of the corresponding Greek of the Rosetta inscription, and by a Greek inscription on the base of an obelisk at Philæ, gave the phonetic characters of the letters which form those words: by their means the names were discovered, in hieroglyphic writing on other monuments of all the Grecian kings and Grecian queens of Egypt, and of fourteen of the Roman emperors ending with Commodus; and by the comparison of these names one with another, the value of all the phonetic characters was finally ascertained.

The hieroglyphic alphabet thus made out has been subsequently applied to the elucidation of the earlier periods of Egyptian history, particularly in tracing the reigns and the succession of the Pharaohs, those native princes who governed Egypt at the period of its splendour; when its monarchy was the most powerful among the nations of the earth; its people the most advanced in learning, and in the cultivation of the arts and sciences; and which has left, as its memorials, constructions more nearly approaching to imperishable, than any other of the works of man, which have been the wonder of every succeeding people, and which are now serving to re-establish, at the expiration of above 3000 years, the details of

its long-forgotten history. To trace these stupendous monuments of art to their respective founders, and thus to fix, approximatively, at least, the epoch of their first existence, is a consequence of the restoration of the knowledge of the alphabet and the language of the inscriptions engraven on them. We propose to review, briefly as our limits require, the principal and most important facts that have thus recently been made known in regard to those early times; and shall deem ourselves most fortunate if we can impart to our readers but a small portion of the interest which we have ourselves derived in watching their progressive discovery.

The following are the authors to whom we are chiefly indebted for the few particulars we know of early Egyptian history. Herodotus and Diodorus Siculus, Grecians, and foreigners in Egypt. Manetho, a native; and Eratosthenes, by birth a Cyrenean, a province bordering on Egypt; both residents. Josephus, a Jew, and Africanus, Eusebius, and Syncellus, Christians, Greek authors. Herodotus visited Egypt four centuries and a half before Christ, and within a century after its conquest by the Persians. In his relation of the affairs of the Greeks and Persians, he has introduced incidentally a sketch of the early history of Egypt, such as he learnt it from popular tradition, and from information obtained from the priests. It is, however, merely a sketch, particularly of the earlier times; and is further recorded by Josephus to have been censured by Manetho for its incorrectness. Diodorus is also understood to have visited Egypt about half a century before Christ; and from him we have a similar sketch to that of Herodotus; a record of the names of the most distinguished kings, and for what they were distinguished; but with intervals, of many generations and of uncertain duration, passed without notice. Manetho was a priest of Heliopolis in Lower Egypt, a city of the first rank amongst the sacred cities of ancient Egypt, and long the resort of foreigners as the seat of learning and knowledge. He lived in the reign of Ptolemy Philadelphus, two centuries and a half before Christ, and wrote, by order of that prince, the history of his own country in the Greek language, translating it, as he states himself, out of the sacred records. His work is, most unfortunately, lost; but the fragments which have been preserved to us, by the writings

of Josephus in the first century of the Christian æra, and by the Greek authors above named of the third and fourth centuries, contain matter, which, if entitled to confidence, is of the highest historical value, *viz.*, a chronological list of the successive rulers of Egypt, from the first foundation of monarchy, to Alexander of Macedon, who succeeded the Persians. This list is divided into thirty dynasties, not all of separate families; a memorable reign appearing in some instances to commence a new dynasty, although happening in the regular succession. It originally contained the length of reign as well as the name of every king; but in consequence of successive transcriptions, variations have crept in, and some few omissions also occur in the record, as it has reached us through the medium of different authors. The chronology of Manetho, adopted with confidence by some, and rejected with equal confidence by others,—his name and his information not being even noticed by some of the modern systematic writers on Egyptian history,—has received the most unquestionable and decisive testimony of its general fidelity by the interpretation of the hieroglyphic inscriptions on the existing monuments: so much so, that by the accordance of the facts attested by these monuments with the record of the historian, we have reason to expect the entire restoration of the annals of the Egyptian monarchy antecedent to the Persian conquest, and which, indeed, is already accomplished in part.

Before we pursue this part of our subject, we must conclude our brief review of the original authorities in early Egyptian history, by a notice of Eratosthenes. He was keeper of the Alexandrian library in the reign of Ptolemy Evergetes, the successor to Ptolemy Philadelphus, under whose reign Manetho wrote. Amongst the few fragments of his works, which have reached us transmitted through the Greek historians, is a catalogue of thirty-eight kings of Thebes, commencing with Menes, (who is mentioned by the other authorities also as the first monarch of Egypt,) and occupying by their successive reigns 1055 years. These names are stated to have been compiled from original records existing at Thebes, which city Eratosthenes visited expressly to consult them. The names of the two first kings in his catalogue are the same with the names of the two first kings of the first dynasty of Manetho; but the

remainder of the catalogue presents no further accordance, either in the names or in the duration of the reigns.

To return to Manetho:—amongst the monarchs of the original Egyptian race there was one named by him Amenophis, (the eighth king of the eighteenth dynasty,) of whom it is stated, in a note of Manetho's preserved by Syncellus, that he was the Egyptian king whom the Greeks called Memnon. The statue of Memnon at Thebes, celebrated through all antiquity for the melodious sounds which it was said to render at sunrise, is identified in the present day by a multitude of Greek inscriptions; one of which, in particular, records the attestation of Publius Balbinus, who visited the ruins of Thebes in the suite of the empress the wife of Adrian, to his having himself heard the "divine sounds of Memnon or Phamenoph;" which latter name is Amenophis, with the Egyptian masculine article φ prefixed, and omitting the Greek termination. The hieroglyphics carved on the statue, and coeval with its date, had been very carefully copied by the French whilst in possession of Egypt, and were engraved in the splendid work, the *Description de l'Egypte*, to which their researches had given rise. These hieroglyphics contain the alphabetic characters Amnf (being the initial vowel and all the consonants of the name Amenof) inclosed within a ring; a distinction which had been previously observed to take place with the names of the Roman emperors, and of the Grecian kings and queens; and as the rings have hitherto been found to occur in no other instance whatsoever than when containing the names and titles of sovereigns, they are regarded as characteristic signs. It should be remarked, that in the hieroglyphic writing, as in the languages of other eastern nations most nearly connected with Egypt, the vowels are often omitted, and when expressed, have not always a fixed sound. The coincidence of the reading of the hieroglyphic name with that recorded by Manetho, and with the Greek inscription on the statue itself, was so far confirmatory of Manetho's authority; it was also highly interesting in the evidence it afforded of the employment of the same hieroglyphic alphabet, that was in after use in the times of the Ptolemies and the Cæsars, even in the very early periods of the Egyptian monarchy; for the reign of Amenophis was in the dynasty preceding that of Sesostris: it also indicated the further

advantage to be gained by the application of the alphabet in decyphering other proper names, distinguished by being inclosed in rings, existing on other statues, and in the more ancient temples generally. Considerable progress had been made in reading these, which in several instances had been found to correspond with the names of the kings of the same and of subsequent dynasties to Amenophis, as given by Manetho, when a most important discovery was made of the existence of a genealogical record, in hieroglyphics, of the titles of thirty-nine kings anterior to Sesostris, chronologically arranged. We have already noticed that the names and *titles* of kings were distinguished by being inclosed in rings; the ring containing the proper name being accompanied usually by a second, inclosing certain other hieroglyphics, expressing the title by which that particular king was designated; and it appears probable that the kings of Egypt were distinguished by their titles rather than by their names, since the same name recurs frequently in different individuals, but the titles are all dissimilar; with a single exception amongst the very many that have come under observation, and in which the same title is common to two brothers. The signification of the titles is yet obscure, except that they are of the same general nature as is frequent in the East, such as "Sun of the Universe," &c.; but for the purpose of individualizing, the sign is to us of the same value as the thing signified; and as other monuments furnish the *names* in connexion with the *titles*, we are enabled to compare the succession evidenced by the titles with the record of the historian, and thus to test the fidelity of the record. The discovery of this hieroglyphic table was made by Mr. William Banks in 1818, in excavating for the purpose of obtaining an accurate ground-plan of the ruins of Abydus, near Thebes. On a side wall of one of the innermost apartments, hieroglyphics were sculptured inclosed in rings, ranged symmetrically in three horizontal rows, each row having originally contained twenty rings, of which twelve of the upper row, eighteen of the middle, and fourteen of the lower row were still remaining, the others having been destroyed by the breaking down of the wall. The hieroglyphics having been copied and lithographed, it was speedily recognised that the rings in the two upper rows consisted of titles only; with the exception of one

proper name, the last of the second row, since known to be the name of the king whose title is the last in the succession, and who was the fourth in reign and generation before Sesostris. The third row was recognised to consist of one proper name and one title, each repeated ten times, and alternating with each other: these are since known to be the name and title of Sesostris, to whose reign the construction of the table is with much probability ascribed. The titles in the same row with that of the ancestor of Sesostris and preceding it, have been identified on other monuments, coupled with names which are those of the predecessors of the same king in the list of Manetho.

It would exceed our limits, and it is not our purpose, to trace in detail the successive steps by which the existence of each of the kings of Manetho's list, from the expulsion of the Phœnician shepherds from Lower Egypt, and the consequent union of Upper and Lower Egypt in a single monarchy, to the reign of Sesostris, has been attested by the monuments. Suffice it to say, that the same number of individuals as stated by Manetho, namely, eighteen, filling a space of four centuries, are shown, by the monuments, to have reigned in that interval, and to have borne the same relationship, as well as succession, to each other, as is expressed by the historian: that, of the eighteen names, eight in different parts of the list are read on the monuments identically as in the historical record; and that in regard to the names that are not identical, we have the testimony of Manetho that some amongst the kings, Sesostris, for example, were known by two and even by more names. The table of Abydus appears to have been strictly a genealogical record; a record of generations, in which view it is strictly accordant with the historian.

The period of the Egyptian annals on which this light has been thrown, is precisely that which might have been selected in the whole history of Egypt as the most desirable for such purpose. Independently of its very high antiquity, it was the period of the greatest splendour and power of the native Egyptian monarchy, and of the highest (Egyptian) cultivation of the arts. The greater part of the more ancient, and by far the most admirable in execution, of the temples, palaces, and statues, which still attest by their ruins their former magnificence, are the work of that age; and the hieroglyphic inscriptions still

extant on them, and which, when not defaced by wanton injury, are almost as perfect as when first executed, make known the reigns in which they were respectively constructed, and frequently the purposes for which they were designed. This is in itself no small achievement, when we reflect that these extraordinary remains of ancient art were equally the objects of vague wonderment in the times of the Roman emperors, as they were in those of the generation preceding ourselves; but that they are become to us objects of a more enlightened curiosity, which they promise amply to repay, when the study that has already made known their founders, shall reveal the signification of the hieroglyphic histories, with which the walls of the palaces and temples are covered. Already have we gained some very important facts in regard to the condition, political and otherwise, of the countries adjoining to Egypt at that early period. The monuments of Nubia are covered with hieroglyphics, perfectly similar both in form and disposition to those on the edifices at Thebes; the same elements, the same formulæ, the same language; and the names of the kings who elevated the most ancient amongst them, are those of the princes who constructed the most ancient parts of the palace of Karnac at Thebes. As far as Soleb on the Nile, 100 leagues to the south of Philæ the extreme frontier of Egypt, are found constructions bearing the inscriptions of an Egyptian king; evidencing that, during the period of which we have been treating, Nubia was inhabited by a people having the same language, the same belief, and the same kings as Egypt. To the south of Soleb, and for more than 100 leagues in ascending the Nile, in ancient Ethiopia, very recent travellers have discovered the remains of temples, of the same general style of architecture as those of Nubia and Egypt, decorated in the same manner with hieroglyphics representing the same mythology, and analogous to those of Egypt in the titles, and in the mode of representing the names and titles, of the sovereigns. But the proper names of the kings inscribed on the edifices of Ethiopia in phonetic characters, have nothing in common with the proper names of the Egyptian kings in the dynasties of Manetho; nor is one of the Ethiopian names found either on the monuments of Nubia or of Egypt. Thus there was a time when the civilized part of Ethiopia,—Meroe, and the banks

of the Nile between Dongola and Meroe,—were inhabited by a people having language, writing, religion, and arts similar to Egypt; but, in political dominion, independent of that country, and ruled by kings of whom it does not appear that any historical record whatsoever has come down to us.

The dates of the expulsion of the Phœnician shepherds from Egypt, and of the reign of Sesostris, in years of the æra of our computation, have been favourite subjects of discussion with chronologists: Archbishop Usher fixed the former of these events in the year B. C. 1825; which would make the commencement of the reign of Sesostris about B. C. 1483. The reign of Sesostris is connected with the early Grecian chronology by the migration of Danaus, brother of Sesostris, who, according to the Parian marbles, arrived in Greece in 1485, which is a very few years earlier than the dates of Usher would assign to that event. M. Champollion Figeac, brother of the M. Champollion to whom the greater part of the discoveries made by the interpretation of hieroglyphics are owing, himself a distinguished chronologist, has assigned the year B.C. 1822 to the expulsion of the Phœnicians, which Usher had placed in 1825: the date of M. Champollion being derived from Manetho's statement, that the Phœnician invasion took place in the 700th year of the Sothiacal period, *viz.*, B.C. 2082, and that their dominion in Egypt continued 260 years. Historical accuracy may make it desirable, that the exact year of the most ancient as well as of more modern events should be determined, if it be possible: but for purposes of general interest, and especially for comparison with the chronology of cotemporary nations, which at that early period is in every case more unsettled than the Egyptian, the period seems sufficiently determined. The date before Christ 1822, pursued downwards through the dynasties of Manetho, conducts with very close approximation to the known period B.C. 525 of the conquest of Egypt by the Persians; and intermediately, accords very satisfactorily with the dates, according to the Bible chronology, of the conquest of Jerusalem in the reign of Jeroboam by Shishak, king of Egypt, and of Tirhakah, king of Ethiopia and Egypt, who made war against Sennacherib; these are the Sesonchis of Manetho, and Sh.sh.n.k of hieroglyphic inscriptions on a temple at Bubaste, and on one of the courts of the

palace at Karnac,—and the Taracus of Manetho, and T.h.r.k of hieroglyphic inscriptions existing in Ethiopia and in Egypt*.

In respect to the connexion of the events of the Jewish and Egyptian histories, the period between the expulsion of the Phœnicians and the reign of Sesostris possesses a peculiar interest, as being that of the residence of the Israelites in Egypt, and of the Exodus. In the history of Josephus, we have an extract from Manetho, in which this latter event is expressly stated to have taken place under the father of Sesostris, a king whose name, in Manetho's list, is Amenophis, (the third of that name,) and on the monuments Ramses. The date which chronologists are generally agreed in assigning to the Exodus is 1491; that of the termination of the reign of Amenophis, according to Champollion, is 1473, or, if the correction of his chronology which we have suggested in a note be just, 1478: it is singular that the difference of thirteen years (between 1491 and 1478) should be precisely the duration of a very suspicious interval which Manetho states to have taken place, after Amenophis had gone with his army in pursuit of the Israelites; and during which interval neither the king nor his army returned to

* It appears to us that a slight inaccuracy has crept into the deduction of all the dates in M. Champollion's Chronology subsequent to the expulsion of the shepherds. The date of that event is the foundation of the subsequent dates, and is supposed to have taken place B.C. 1822; after which, according to the extract of Manetho in Josephus cited by M. Champollion, Thoutmosis, the king by whom they had been expelled, reigned 25 years and 4 months, followed by the other kings of the eighteenth dynasty, making altogether 342 years and 9 months: (including the 2 years and 2 months additional of Horus, in compliance with the version of the passage in the Armenian text of the Chronicle of Eusebius.) This number, 342 years and 9 months, falling short of the 348 years attributed to the eighteenth dynasty in Eusebius and Syncellus, M. Champollion has suggested that Thoutmosis may have reigned the five years which constitute the difference, before the expulsion of the shepherds, since, according to the record, he did reign, some years before that event, over all the parts of Egypt not possessed by the shepherds. So far, so well: but in such case, the year B.C. 1822, being the epoch of the expulsion of the shepherds, and not of the commencement of the eighteenth dynasty, must surely correspond to the fifth year of the reign of Thoutmosis, and not to the first, as M. Champollion makes it. We have hesitated to venture this remark on a matter to which M. Champollion must have given so much attention, believing that mistake in us is much more probable than an accidental inadvertence in him; but we have returned frequently to the consideration, without having been able to satisfy ourselves; and the rectification of our mistake, if it is one, may prevent others falling into the same.

Egypt, but are stated to have been absent in Ethiopia. If the Exodus occurred during the reign of any of the kings of the eighteenth dynasty, it could only have been in the reign of the immediate predecessor of Sesostris; since his conquests in Phœnicia, and his expeditions against the Assyrians and Medes, must have brought him in contact with the Israelites, had they been then residing in the Holy Land, so as at least to have caused some mention to have been made in their history of the passages of so great a conqueror. But presuming Amenophis, father and predecessor of Sesostris, to have been the Pharaoh of the Exodus, the wandering of the Israelites in the desert for forty of the fifty-five years ascribed to the reign of Sesostris, is a sufficient explanation of his being unnoticed in the Jewish history; whilst the fact of that nation having been subject to the Egyptians during the reign of Ousirei, commencing 124 years before the death of Amenophis, is attested by the paintings on the wall of one of the chambers of the tomb of that king, discovered by Belzoni, and with which we are so well acquainted by means of the model exhibited in England.

Whilst recalling to recollection the peculiar physiognomy of the Jews pourtrayed in that tomb,—and which is as characteristic of their present physiognomy as if it had been painted in the present age, instead of above 3000 years ago,—the equally well characterized, but very different physiognomy of the Phœnician shepherds, represented on the monuments of the same period, is decisive of the error of Josephus, who imagined the Jews and the Shepherds to be the same people. The Phœnician shepherds, long the inveterate enemy of the Egyptians, form a leading feature as captives, in the representations of the exploits of the monarchs who conducted the warfare against them. These people are always painted with blue eyes and light hair; and it is not a little curious to see assembled on the wall of the same apartment, different races, so distinctly characterised as the Jew, the Phœnician, the Egyptian, and the Negro; the latter in colour, and in the outline of the features, in painting and in sculpture, precisely as at present; all, moreover, inhabitants of countries not very distant from each other, and at a period when not more than twelve or thirteen centuries had passed since all these races had descended from a single parent. In the writings which attempt to explain from natural causes

the diversity of race amongst mankind, much power has been ascribed to the effects of time and climate: but the facts with which we are now becoming better acquainted than before, do not appear to admit of explanation from those circumstances. It is worthy of notice that the negro, and the light-haired and blue-eyed people, the two races who might be deemed at the greatest distance apart amongst the varieties of man, are, equally with the intermediate Egyptians, the descendants of Ham.

Of the succession of kings in Manetho's chronology, from Sesostris to the Persian conquest, a space of nine centuries and a half, about one half the names have been already identified on different monuments: four of the Persian monarchs, subsequent to the conquest, have also been traced in inscriptions in phonetic characters; their names are written, as nearly as can be spelt with our letters, Kamboth, (Cambyses); Ntariousch, (Darius); Khschearscha, (Xerxes); and Artakschessch, (Artaxerxes.)

The ascent by monumental evidence to yet more remote antiquity than the expulsion of the Phœnician shepherds, (B.C. 1822), is not altogether without hope, notwithstanding the general demolition of the temples of the gods, which took place according to Manetho, during the long dominion of the Phœnicians in Egypt. We learn from the *Description de l'Egypte* that even the most ancient structures at Thebes are themselves composed of the debris of still more ancient buildings, used as simple materials, on which previously sculptured and painted hieroglyphics are still existing; these are doubtless the remains of the demolished temples, but the inscriptions will require to be studied on the spot. There is also reason to believe, that there exists amongst the ruins of the palace of Karnac, a portion of still more ancient construction than the palace itself; which, having escaped demolition, was incorporated with the more recent building. The inscriptions on this apparently very ancient ruin present the name and title of a king, which form a very interesting subject for future elucidation. The title does not accord with any one now extant on the table of Abydus, but possibly may have been one of those which were destroyed with a portion of the wall, and which are of kings of earlier date than the expulsion of the shepherds. The name is Mandouei, which name occurs in the dynasty anterior to Sesostris, but coupled

with a different title, an effectual distinction; nor does the name recur in any subsequent dynasty. M. Champollion Figeac has, with much ingenuity, shown the probability of the identity of the Mandouei of the ancient ruin with the Osymandyas, Ousi-Mandouei, mentioned by Diodorus Siculus as an Egyptian king greatly distinguished by his conquests, whose reign M. Champollion infers, from the historical passages relating to him, to have commenced 190 years before the Phœnician invasion, or B.C. 2272 years; a prodigious antiquity, and of the very highest interest should it be established, since there exist of this individual no less than three statues in European collections, distinguished by the same name and title: two of these are colossal, one at Turin, and a second at Rome: a third is in the British Museum; and as all particulars must interest which relate to a statue, of which there is at least probability that it is the most ancient existing in the world,—the date attributed to it being earlier than the birth of Abraham,—we copy from Burckhardt the following short description of its discovery: "Within the inclosure of the interior part of the temple at Karnac, Belzoni found a statue of a hard, large-grained sandstone: a whole length naked figure sitting upon a chair with a ram's head upon the knees: the face and body entire; with plaited hair falling down to the shoulders. This is one of the first, I should say, the first Egyptian statue I have seen: the expression of the face is exquisite, and I believe it to be a portrait."—(J. L. Burckhardt, *Travels in Nubia,* lxxvii. *Letter to Mr. W. Hamilton, 20th February,* 1817.)—This statue is in the farthest corner on the right hand side after entering the gallery of the Egyptian antiquities in the British Museum; and compared with other statues in the same gallery, which are of kings of the eighteenth dynasty, the dissimilarity of the features from the very characteristic ones of the latter family is too striking to be questioned. The problem of the age of this king Mandouei is, at all events, a highly curious one; and will probably receive its solution amongst the many other valuable discoveries which cannot fail to result from M. Champollion's projected visit to Egypt, in which he will be accompanied by the sincere good wishes of every one in every country, who feels an interest in the restoration of authentic history. E. S.

Proceedings of the Horticultural Society.

June 19th.

At this meeting a paper was read from the President, T. A. Knight, Esq., upon the culture of the mango and cherimoyer. Its object was to suggest some improvements in the management of these and other trees cultivated in stoves, deduced from an application of Dutrochet's electrical theory of vegetation to practice. It has now become generally known that this observer is of opinion that the motion of the fluids in plants depends upon two currents of electricity, setting with very unequal force between the denser fluid of the tree and the lighter fluid of the soil in which the tree is planted; the more powerful current setting from the latter to the former, and so producing absorption, by conveying aqueous particles into the roots, through the vegetable membrane of the epidermis. In applying this theory to practical purposes, Mr. Knight recommends that the pot in which the cherimoyer or mango is planted, should itself be surrounded by a medium through which an equable and regular supply of fluid may be conveyed to the roots, and that the naked surface of the pot should by no means be exposed to the free action of the atmosphere. Without entering upon any question of the accuracy of the French philosopher's observations, it is quite certain that such a mode of cultivation is that which is most congenial to plants, and which is indispensable to those of a habit at all delicate. The common practice of plunging pots into a tan-bed, or among sand, if in glass-houses, or in the earth if in open borders, is a proof of the necessity that gardeners have found, of securing as regular a temperature and degree of humidity as is possible for the outside of their flower-pots; through the pores in which, moisture is chiefly conveyed to the roots, which always cling to the inside surface of the pot.

Specimens of roses produced by branches budded upon the Rosa indica, were exhibited by Alexander Evelyn, Esq. We notice these not only on account of their extraordinary beauty, but also for the sake of recommending most strongly the adoption of the practice where delicate roses are found difficult of cultivation *per se.* If we consider what happens when the operation of budding, or grafting has succeeded, the reason of the advantage derived from such an operation will be apparent. When a bud of one variety is inserted under the bark of another variety, a union takes place between the cellular substance of the two; the bud is then placed in the same

situation with regard to the stock, as the seed when sown is with regard to the earth. It immediately derives its nutriment from the ascending sap of the new tree, and begins to form its wood and branches, and to secrete its proper juices in proportion to the supply of food it now receives. If a plant from any cause produces roots with difficulty, its whole habit will be delicate, and its flowers if formed, will, as in the case of that most lovely of flowers, the double yellow rose, probably fall off without expanding, from the want of an adequate supply of nutriment from its roots; but, as in all trees, every bud is, when fully formed, in itself a perfect and distinct individual, if such an individual be removed from its own root, and placed where it will be supported by the healthy vigorous roots of another species of variety, which happens in budding, it will no longer have to depend upon a source, the supplies from which are imperfect, but on the contrary, like a seed removed from barren to fertile ground, it will flourish in a degree before unknown. The contrary effect takes place when a vigorous plant is transferred to one less vigorous. And hence, the whole effect of stocks upon the scions, or buds inserted upon them.

There was also a great variety of fruit and flowers upon the table, and seeds of several useful vegetables were distributed.

July 3rd.

Seven medals were awarded to different individuals for fruit sent by them to the Society's fête on the 23rd of June; and one to Capt. Drummond, for his "successful exertions in bringing living plants of the mangosteen from the East Indies." A paper by the president was read upon an improvement in the mode of constructing hotbeds, but we despair of explaining it successfully without reference to figures. Among the display of fruits and flowers, which were exceedingly numerous, we were particularly struck by a collection of twenty-two varieties of strawberries from the Society's garden.

Upon this occasion, thirty-nine new members were either balloted for, or proposed, a striking proof of the estimation in which the Society is held by the public.

July 17th.

Upon this occasion, an enormous pine-cone from the River Columbia was exhibited. It measured $16\frac{1}{2}$ inches in length, and was stated to have been procured by the Society's collector, Mr. David Douglas. Its seeds were represented to be as large as those of the stone-pine, and eatable. The tree is of the family of Pinus strobus,

and will be an invaluable acquisition to our forests, if it should prove to succeed as well in this climate as in its own. We have already given some account of this plant in the last number of the old series of this Journal. The usual display was made of the finest fruit and flowers of the season.

August 7th.

A complete coloured set of the costly Flora danica was placed upon the table, having been presented by His Majesty the King of Denmark. An improved apparatus for fumigating hothouses was exhibited by its inventor, Mr. John Read: it consists of a brass cylinder, attached to the orifice of a pair of bellows, and fitted up with a chimney and draft-hole closed by a valve. The tobacco is put into the cylinder and ignited, and the blast from the bellows expels the smoke. The contrivance is ingenious enough, but while a hothouse fifty feet long, may be filled with smoke in ten minutes by means of a flower-pot, with a hole in its bottom, and a common pair of bellows, we cannot recommend any more expensive, and certainly less efficient apparatus.

The table was covered with a profusion of fruits and flowers.

August 21st.

The meeting-room this day exhibited a gratifying proof of the excellence of the productions of our English gardens. Of *flowers*, there were dahlias of the richest colours, and the most varied hues; some produced by plants that retain all their ancient stature, and others by dwarfs which seem to have lost nearly every character of the dahlia but its beauty. Of *fruits*, there were endless varieties of apricots, apples, pears, peaches, nectarines, grapes, pine-apples, and melons; one of the latter, from the garden of John Fuller, Esq., weighed thirteen pounds. The best apricot was the Moorpark; the best apple, the Duchess of Oldenburg, than which no princess has a fairer bloom, the best pear the Jargonelle, the best peach the Bourdine (forced), the best pine apple the Black Jamaica. We mention these as a guide to our readers, in their purchases of fruit-trees; for it is certain, that no greater service can be rendered to the public, than to point out the means by which they may avoid encumbering themselves with the polyonymous trash with which every nursery abounds.

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *On the combined Action of a Current of Air and the Pressure of the Atmosphere.*—The phenomena observed by M. Clement Désormes *, when a flat plate is opposed to air or vapour passing into the atmosphere from an aperture in a plane surface, have been rendered so easy of production by M. Hachette, as to be at the command of any person in any situation. M. Hachette has also accompanied the description of his instruments with elucidations, experiments, and philosophical reasonings.

The first simplification by M. Hachette was to make the nozzle of a pair of double chamber-bellows terminate in the middle of a flat plate; he found that when the bellows were worked, effects were produced opposite the jet of air of the kind described by M. Clement, disks of card and other substances being drawn towards the aperture against the direction of the current. At the same time that he described this experiment, he also announced his having produced the same effects by using a stream of water instead of a stream of air.

The apparatus was still further simplified, so as to make the stream of air from the mouth sufficient to produce the effect. A

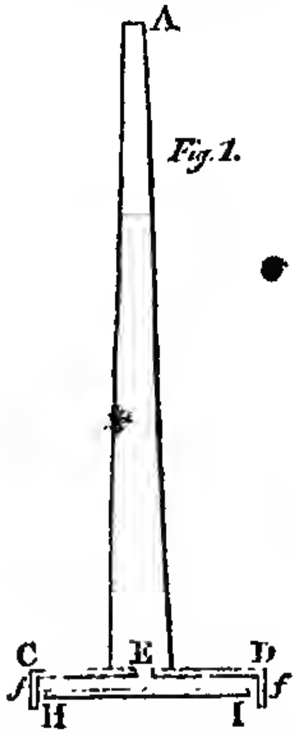


Fig. 1.

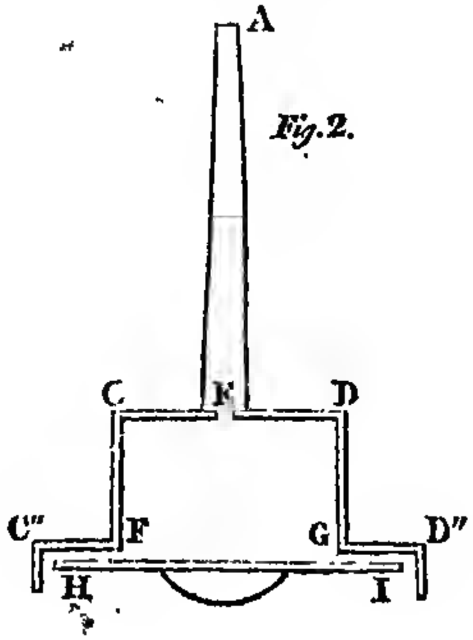


Fig. 2.

tin tube, A, *Fig.* 1, was soldered to the middle of a round tin plate, in the centre of which was a small orifice, E; three or four small projections of the tin, *f f*, were left at the edges of the plate, to prevent the disks of paper, card, or metal, from slipping off sideways. The figure is on a scale of one-half. Instead of the tin plate, a piece

* See the last volume of this Journal, p. 473.

of smooth cork may be used, and for the tin tube, a glass tube, or one made by rolling up a piece of paper.

If the tube be held horizontally, or inclining a little upward, and a disk of card or paper be placed loosely against the aperture in the plate, it will be found that, on applying the mouth to the end of the tube, and blowing air through, that the disk will not be driven away, but actually made to apply closely to the surface of the plate; and if turned towards the ground it will be found to remain opposite the hole, and not to fall until the current of air is stopped. Even a plate of tin may in this way be suspended by a current of air; which at first would be supposed to conjoin with gravity in forcing it to the ground. When the disk is flexible and slightly elastic, a heavy sound, and sometimes even a shrill tone, is produced by the vibrations of the plate.

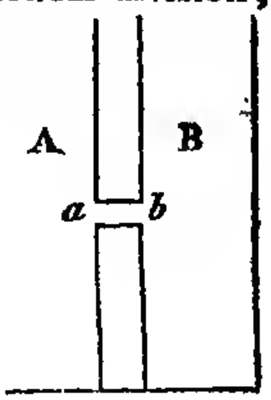
In explanation of this experiment, M. Hachette says, "The air is pushed from the mouth A of the tube, towards the orifice E of the plate; it strikes the part of the disk opposed to this orifice, and the mean pressure on that part is greater than the pressure of the atmosphere. The blown air then takes place of that between the plate and the disk opposed to it; it moves in this interval with a velocity decreasing from the edges of the aperture: the elastic force of this air decreases at the same time, so that its mean pressure between the plate and the inner face of the disk becomes less than the atmospheric pressure; and as this last pressure is exerted on the whole external face of the disk H, I, this disk, subject at the same time to the two contrary pressures on its opposing faces, obeys the greater, and is pushed towards the plate C D.

It is not necessary that the disk, C D, should be near the orifice E, of the tube A E. Let *Fig.* 2 be an instrument composed of a hollow cylinder, C D F G, and a flat border of the dimensions C″ F, or G D″. Let a tube, A E, be fixed to the bottom of the cylinder, the orifice E having a diameter of about three millimeters (0.12 of inch). If air be blown in at A, against the disk, H I, in the neighbourhood of the flat border, the disk will be urged towards the orifice E. This instrument is also delineated on a scale of one half. The disk, with the attached weight, weighs about 12 grammes (184.87 grains), being 54 millimeters in diameter; the pressure of the atmosphere upon it equals 23 kilogrammes: from which it follows that, in this experiment, the pressure of the air blown upon the inner surface of the disk, and the atmospheric pressure exerted on the exterior of the same disk, only differs from each other by about one two-thousandth part of the latter.—*Annales de Chimie*, xxxv. 34.

2. *Considerations relative to Capillary Action, by* M. Poisson.—M. Dutrochet, whilst explaining his views relative to the cause of vital movement in plants and animals, stated that if an animal or vegetable membrane were formed into a bag, having a tube of

glass attached to its aperture, and were then filled with a liquid substance, having a strong affinity for another liquid, into which the bag was to be immersed, it would not only have the power of absorbing the latter liquid into its pores, but also, in certain cases, of forcing it up to the top, and even out of the glass tube held in a vertical position. On this point, a difference of opinion with regard to the force of capillarity took place: M. Ampère maintaining that capillary action would raise the fluid to the top of the tube, but not cause its expulsion; while M. Poisson maintained that, in certain cases, the latter effect could be produced. The latter has since then published a note, which we transcribe in part, from the *Annales de Chimie*, xxxv. 98.

Suppose that two different fluids, A, B, are contained in a vessel, and separated the one from the other by a vertical division; the heights being in an inverse ratio to the densities, so that the points, *a* and *b*, in the two faces of the division, and situated in the same horizontal plane, shall support equal and opposite pressures: suppose also that the division is pierced with one or more holes of small diameter, or, in other words, that it is traversed by several very narrow canals, as *a*, *b*, perpendicular to the two faces, and which may be regarded at first as filled with air, or any other fluid.



If the substance of the division exerts upon each of the two liquids an action superior to the half of that which the liquid has upon itself, each liquid will enter into the canal *a*, *b*, just as it would rise above its ordinary level in a capillary tube of the same size and substance. It would also be urged, by the excess of pressure which it would exert at the extremity of the canal, against the elasticity of the included air. When the two fluids have penetrated the interior of *a*, *b*, the air will be pushed on both sides in different directions by forces each of which is equal to the primitive pressure augmented by the corresponding capillary force, *i. e.* augmented by forces proportional, according to the known theory of M. Laplace, to double the action of the tube on the liquid, less the proper action of the liquid itself. It will only be in the case when the capillary force shall be the same on both sides, that the air, after being compressed to a certain degree, will remain at rest: for whenever this force preponderates at one end of the canal, the air will be driven out at the opposite end, and the liquid with the strongest capillary attraction will entirely fill the canal.

Suppose this liquid to be A, then let us consider the forces which will act on the portion *a*, *b*, of this liquid. At the extremity *a*, it will be submitted to the attraction of the exterior fluid A: at the extremity *b*, it will be attracted in the opposite direction by the liquid B. Now the two liquids being different, their attractions will be unequal, and we will suppose that that of B, on the matter

of A, is greater than that of A for itself. As to the action of the canal on the portion a, b, that will be equal, and exerted in contrary directions at its two extremities; it will not, therefore, be either adverse or favourable to the movement of the fluid in the canal: and the same will be the case with respect to the pressures exerted at a and b, by the external liquids, as long as they are equal: nevertheless, the action of the canal, and the external pressures, will prevent the thread of fluid from being broken, so that it will move without interruption in the direction in which it is drawn by the greatest attraction, or from a to b. Hence will result an elevation of the level of B, and, consequently, an increase of pressure at the extremity b, of the passage, and this elevation will proceed until the difference of pressure in a and b shall be equal to that of the attractions exerted by the two fluids A and B, on the thread $a\ b$; this effect will be produced the more rapidly as the division is pierced with a greater number of passages similar to that which has been considered.

Now let us examine what would occur if the division were formed of two others different in their nature, and exactly superposed; exerting no action on one of the liquids, B for example, and one only acting on the other liquid. The liquid B will then retain its original position undisturbed; in consequence of the action it exerts upon itself it cannot penetrate the canal $a\ b$, just as mercury cannot escape by a capillary aperture made in a barometer-tube. It will be the same with A, when that face of the division which exerts no action upon the liquid is turned towards it; so that how numerous soever the apertures, the two liquids would, under such circumstances, remain separate and preserve their original level. But if the division be turned so that the face which acts upon A shall be in contact with that liquid, it will penetrate the canal $a\ b$ by means of capillary attraction; and the velocity which the liquid urged by this force may acquire, may make it pass that point in the canal where the division changes its nature, and even make it reach the extremity in the liquid B, so that it is possible that the liquid A should entirely fill the canal $a\ b$, as in the case which has already been examined. Then if we always suppose the attraction of B for A to be superior to that which A has for itself, the thread $a\ b$ will flow into B until the level of the latter is so far altered that the excess of pressure at b can balance the difference of attractions exerted by the two liquids at a and b.

M. Poisson then observes that, without pretending to assign a cause, exclusive of all others, for the phenomena of absorption by vegetable and animal membranes observed by M. Dutrochet, his object is to show that effects which have at least a great resemblance to these important phenomena, may be produced by capillary action conjoined with the difference of affinity existing between heterogeneous substances without the assistance of electricity, either moving or quiescent. It appears that M. Dutrochet afterwards

found mineral substances, as a piece of slate, might be substituted for the organized tissues; this being the case, the opinion which refers such effects to a general cause, as capillary attraction, acquires more probability.

3. *Novel Use of the Plough.*—Mr. Bruckmann states that he has long thought the plough might be used in levelling roads and clearing the foundations for fortifications. In 1824 he had an opportunity of applying it in the construction of a canal required to furnish a motive force for the service of the rock-salt works of Friedrichshall. The bed was to have a section of 700 square feet, and it had been calculated that the excavations would require 200 men for two years, whereas the king of Wurtemburg wished it to be done in one year from the spring of 1824.

Three ploughs were employed; the first had two handles, a coulter, and a share, the latter being in the form of a wedge. This plough was preferred in the beds and gravelly grounds; and it was found advantageous to give it an oscillatory movement by the handles during its progress. Drawn by eight horses, it could turn up 25,000 cubic feet of an argillaceous soil, in three hours; with ten horses it turned up 19,800 cubic feet of a gravelly soil, in the same time. This plough was tried in 1815, against fifteen others of the ordinary kind, in the construction of a watercourse for a mill; all the fifteen were quickly broken by the work.

The second plough had two handles and a coulter, but the share had only one cutting edge, which was rounded and with an ear. It was made five times as strong as an ordinary plough, and succeeded well in compact and argillaceous soils, where, with eight horses and four men, it moved 48,000 cubic feet of earth in three hours. In case of fracture ten minutes sufficed to change the coulter and share, and, during the work, 2,300,000 cubic feet of earth were loosened by it.

The third plough was smaller and lighter, it had two handles, a coulter, an ear, and a share, the latter lance-shaped. It was used for excavating the sides of the canal, on which the horses attached to the first plough found it difficult to walk because of the inclination. It was worked by ten or twelve men.

To establish an accurate comparison between the work of these ploughs and that done by the pickaxe and spade, a piece of ground was wrought solely in the latter manner by six strong working men. The result of a long trial was the breaking of 150 cubic feet of ground by each man in nine hours. Comparing this result with the work of the ploughs, the following are the results:—The first plough did the work of 477 men, the second of 960 men, and the third that of 50 or 60 men. The canal was finished on April 30th, 1825, the ploughs having saved 32,000 days, according to the work-day of a labourer.—*Bull. Univ.* D. vii. 343.

4. *Discovery of Rocks under the Surface of the Sea.*—The fishers of the Mediterranean use an apparatus for the discovery of rocks beneath the surface in those places where they wish to cast their nets, which supplies, in a great measure, the insufficiencies of the ordinary means of taking soundings. The method consists in carrying a long and thin cord over the bottom to be examined, and which, when it meets with an obstacle, is stopped by it and becomes folded in the place where it occurs. It will easily be understood, that when a cord has been carried over a certain space without meeting with any resistance, that proof is obtained of the non-existence of rocks or other obstacles, at a depth less than that to which the cord has been sunk; and as the examination can easily be carried on to 100 feet below the surface, it may be said that, wherever such an apparatus has passed unimpeded, the navigation is free. If, on the contrary, some isolated rocks are found during the examination, the place where the cord becomes doubled points out the locality, which may then be determined more accurately by other trials, and the summit and neighbourhood of the submersed rocks be accurately examined by means of soundings.—*Annales Marit.*; *Bull. Univ.* F. viii. 44.

5. *Paper to resist Humidity.*—This process, which is due to M. Engle, consists in plunging unsized paper once or twice into a clear solution of mastic in oil of turpentine, and drying it by a gentle heat. The paper, without becoming transparent, has all the properties of writing-paper, and may be used for the same purposes. It is especially recommended for passports, workmen's books, legal papers, &c. When preserved for years it is free from injury, either by humidity, mice, or insects. It is further added, that a solution of caoutchouc will produce even a still better effect.—*Kunst und Gewerbe-blatte.*

6. *Professor Amici's Microscopes.*—This distinguished personage has lately exhibited to the *savans* of this country two microscopes of his own workmanship,—an achromatic refractor, and a reflector of his own particular invention. The object-glass of his refractor is of a very complicated construction, and is composed of three double-object glasses combined together in the space of about an inch. The flint-glass from which his concaves are formed is of the manufacture of Frauenhofer; his convexes are of Dutch plate, crown-glass, and French plate, separately. Each object-glass detached has but a small aperture, and is of long focus; but when the three are combined together, the angle of aperture is very considerable, and the focus short. By this ingenious arrangement the trouble and difficulty of manipulating deep single-object-glasses of large aperture is avoided; but advantages gained one way, in practical optics, are generally lost in another, and the twelve surfaces of the objective produce a kind of softness and muddiness in

the image strongly contrasted by the effect of a good single triple-glass of equivalent power. When, however, only two of the object-glasses are combined, the effect is very fine. Between the object-glass and eye-glasses is placed one of those prisms originally invented by Sir I. Newton to act as an eye-piece of his telescope, and of which a description may be seen in his correspondence at the end of Dr. Gregory's Optics. The utility of the introduction of this device appears very questionable in an instrument already so complicated. The diversion of the rays into a course at a right angle to their original progress (merely to give an horizontal instead of a vertical position to the body) is surely no warrant for the employment of two extra refracting surfaces and one reflexion, which cannot fail to have a pernicious influence on the formation of the image. An horizontal position of the body is attained with the utmost facility by a proper construction of the mounting, &c. Setting aside the dulness of the image produced by the numerous refractions, the performance of the instrument on test-objects was highly respectable and satisfactory.

The reflector is a modification of the original construction recommended by the Professor, who seems to have profited by the schooling he received from Dr. Goring, and now sails much closer to the wind than he did. His objective metal is now two inches focus, with an aperture of $1\frac{1}{2}$ inch; but half an inch is cut off for the purpose of preventing the bad effect of the marginal rays, so that only 1 inch of the central portion of the metal is employed;—the diameter of the diagonal mirror is also reduced to its proper standard, by which means the blot in the centre of the visual pencil is rendered as small as possible. It may be asserted of this instrument, that it does as much as can possibly be expected from an objective part of 2 inches focus, showing many test-objects faintly, and with much effort; but it is totally unable to compete with deeper ones equally perfect and of the same angular opening. The Professor has, in some of his instruments, reduced the focus of the elliptic metal to $1\frac{1}{2}$ inch, and will, no doubt, gradually slide into the adoption of that radical reform in his instrument, so happily carried into effect in this country by Dr. Goring, in conjunction with Mr. Cuthbert,—at least if the figuration of elliptic metals of $\frac{3}{10}$ inch focus with $\frac{2}{10}$ inch of aperture shall not surpass his powers of execution. During the Professor's stay in this country there was a grand field-day at his hotel, at which *both his microscopes were tried against the Goringian modification of the reflector, the superior weight of metal of which completely beat every thing opposed to it.* For the honour of the Professor it must be stated, that he admitted this defeat with great candour and good sense, and even had some difficulty in believing in the identity of some of the objects used, so differently was the ordinary apparent structure developed by the English improvements on his instrument. It may with safety be averred that no refractor, at least, will ever be

made to surpass *Dr. Goring's improved Amician Engiscope;* and it seems equally certain that no other reflector will ever be invented capable of the same facilities of application to the examination of both opaque and transparent objects. If Professor Amici has been beaten, it has been done with his own weapons,—the copy has surpassed the original,—the child, by virtue of foreign nursing and tuition, has exceeded the stature and strength of the father.

II. Chemical Science.

1. *On the Specific Heat of Gases, by* MM. de la Rive *and* Marcet.—The principle on which these philosophers proceeded in their researches, was, to expose equal volumes of different gases to an equal source of heat during equal times, and to judge, by the augmentation of elastic force in each gas, the temperature which it had acquired. The apparatus was a kind of manometer, and consisted of a glass balloon to retain the gas, and a bent tube attached to it, which, descending into a vessel of mercury, served to show, by the column of metal within it, what was the elasticity of the gas. This method was adopted, because, i. The gas was not altered in volume by the change of temperature, its elasticity only changing: ii. The temperature was indicated by the gas itself, and not by a thermometer: iii. Water was easily separated previously from the gases, and excluded from the apparatus: iv. All the gases were placed in exactly the same circumstances, so as to render it unnecessary to refer to any calculation for the purpose of comparison.

Two methods of applying heat were resorted to: in one the balloon, containing the gas at a certain temperature, was placed in water at a higher but constant degree, for a certain time (generally 4″), and the elevation of temperature noticed: in the other, the balloon with the gas was inclosed in a larger copper balloon, blackened inside, and the space between the two exhausted as much as possible of air; the apparatus being then immersed in warm water, the heat gained access slowly to the gas, and the time of each experiment was increased, at the same time that certain sources of error were avoided.

The gases experimented with were, atmospheric air, oxygen, azote, hydrogen, carbonic acid, olefiant gas, oxide of carbon, nitrous oxide, nitrous gas, sulphuretted hydrogen, ammonia, sulphurous acid, muriatic acid, and cyanogen. Great care was taken in their preparation. The result of the experiment was very unexpected; for, during the five minutes allotted for each, all had acquired the same temperature,—a circumstance which proves that they all have the *same specific heat.* The equal volumes of gas at the pressure of 65 centimeters (15.59 inches) and the temperature of 20° C., being exposed to a source of heat at 30° C., acquired a mean temperature of 6.32 degrees in five minutes, the extreme difference in any of the experiments, not being more than 0.04 of a

degree. One gas only forms an exception to the above statement, namely, hydrogen, which was always heated more than the others, namely, to 6.6 degrees in the five minutes. This effect is considered as due not to any difference in specific heat, but to a difference in conducting power.

Experiments were then made with dilated gases to ascertain whether dilatation caused any change in capacity, and it was found to diminish slowly but regularly with the diminution of pressure. These results, with a third which is also interesting, have been thus generally expressed by the authors at the end of their memoir.

i. All gases in equal volumes, and at the same pressure, have the same specific heat.

ii. Other circumstances being the same, the specific heat of gases diminishes with diminution of pressure, and equally for all the gases: the progression converges slightly and in a ratio much less than that of the pressures.

iii. Each gas has a different conducting power, *i.e.*, all the gases have not the same power of communicating or receiving heat.—*Ann. de Chimie*, xxxv. 5.

2. *On the Incandescence and Light of Lime.*—The experiments made by Lieutenant Drummond upon the light of lime and other earths when highly ignited, with the highly interesting application which he has made of that emitted from lime, to the purpose of geodesical surveys, has induced M. Pleischel to repeat and vary the results. He states that the utmost light is given by lime; the earth being pulverised and exposed on burning charcoal to the heat excited by a jet of oxygen falling upon it. He endeavours to account for the effect, by supposing a kind of pulverulent atmosphere disengaged from the lime at the high temperature used, and considers that the substances which are competent to emit molecules only in the gaseous state, cannot produce this intense light.—*Zeitschrift für Physik*, &c.

3. *Evolution of Heat during the Compression of Water.* May 14, 1827.—M. Arago announced to the Academy of Sciences, that M. Despretz had ascertained experimentally, that the compression of water by a force equal to 20 atmospheres, caused the disengagement of one sixty-sixth part of a degree of heat.

4. *On Electrical Excitation.*—M. Walcker affirms positively from experiments made with great care, that three bodies of different exciting power are necessary in every case of excitation of electricity by contact, and that all the phenomena of this kind are subject to this condition. If, for instance, two portions of the same metal being put in contact, electricity is produced, it is because there are three different states of temperature brought into play, one being the result of the other two, and a mean between them. One fact which more than any other sanctioned this idea, was, that the elec-

tric currents were the more apparent as this third state of temperature was made more sensible.—*Bull. Univ.*, A. vii. 374.

5. *Magnetic Repulsion.*—A very remarkable result has been obtained by M. Becquerel, from the use of an extremely delicate magnetic arrangement, which he has for the present called a *sideroscope.* Its use is exactly the same in principle as that of the magnetic needle, indicating iron, for instance, by the attraction manifested; but it is so delicate that it will show it in the most minute quantity possible, as, for instance, in gold, silver, or copper money, innumerable minerals, &c. This instrument shows no magnetic power or attraction in gold, silver, copper, palladium, tin, lead, zinc, or brass, when chemically pure, and a great many vegetable and mineral substances have no action on it: but the most curious result is, that very pure bismuth and even that of commerce has a *repulsive* power, which, if it be found ultimately to be independent of any magnetic *polarity*, is the first fact of the kind that has been made known. Antimony also presents the same phenomenon.

6. *Diminished Solubility of Substances by Heat.*—Mr. Graham has added one to the few facts of this kind with which we were acquainted, and has accompanied its description with some very interesting considerations, which may be found at length in the Philosophical Magazine, N. S., ii. 20. The salt experimented with by Mr. Graham is the phosphate of magnesia; which may be prepared by mixing a solution of 21 parts of phosphate of soda with one of 15.375 parts of sulphate of magnesia: within 24 hours the phosphate of magnesia precipitates in acicular crystals; they should be agitated with repeated portions of water, then thrown upon a filter with more water, and left to dry.

Solutions were obtained by occasionally agitating this salt with water in the proportion of 2 ounces to a pint of the fluid, for four days; being then decanted and filtered, they had a sweetish taste. A quantity of this fluid being heated in a water-bath, became turbid before the temperature had attained 120° F.; at 212° a cloudy precipitate slowly subsided, and the supernatant fluid became nearly transparent. The precipitate was found to be anhydrous phosphate of magnesia; and, by further experiment, the difference in solubility was found to be such, that water at 45°, dissolving $\frac{1}{7344}$th part its weight of the anhydrous salt, water at 212° only dissolved $\frac{1}{11351}$th part. When in the state of crystals, or as hydrate, the proportions of salt were $\frac{1}{322}$ and $\frac{1}{498}$ to 1 of water.

Mere continuance of the heat had no effect in increasing the precipitate either of this salt, or from aqueous solution of lime, provided no part of the solution was at any time converted into vapour; but if the solution only occupied a small part of the vessel, and ebullition came on, then, although all the water might be returned to the solution, yet the precipitation went on, and might be

increased *ad libitum*, particularly in the case of lime water. The cause of the precipitate appears to be the same in all these cases. The moment a drop of the solution is converted into vapour, it deposits the quantity of lime or salt which it held in solution; and in the case of bodies which dissolve so sparingly and with so much difficulty, although the water be returned again to the solution, it is incapable of re-dissolving what it has deposited. We know that it would be a hopeless task to form a saturated solution of lime by agitating with the water no more than the few grains which it is capable of dissolving; and in the case of ebullition, when the lime is once deposited, there should be the same difficulty in taking it up.

Mr. Graham states that he has observed this effect not only in lime-water and in solution of phosphate of magnesia, but to a certain extent in all bodies of difficult solubility, in the sulphate of lime, for instance, even when greatly diluted; and he believes that the deposite from slight boiling observed in many mineral waters, and generally attributed to the dissipation of carbonic acid gas, depends, in some instances, upon this cause. However weak the solution may be, it is evident that a portion of the salt may be deposited in this way.

7. *On the Composition of Cyanic Acid.*—M. Wohler some time since announced the production of cyanic acid, and cyanates, corresponding in composition to the substance presumed to exist in the fulminating compounds of silver, mercury, &c., the nature of which was made out by MM. Liebig and Gay Lussac. M. Liebig, upon repeating M. Wohler's experiments upon his cyanate of silver, obtained only 71.012 per cent. of oxide of silver, instead of 77.23, which was the quantity present according to M. Wohler's analysis, and concluded that the acid was the cyanous, and not the cyanic. The latter philosopher was consequently induced to repeat his experiments: one of his methods of decomposing the cyanate of silver was by muriatic acid gas: at first liquid cyanic acid forms, which is very soon transformed into a white crystalline mass; but, on continuing the operation, and applying a higher heat, a large quantity of muriate of ammonia and cyanic acid is evolved. This process indicated 77.5 per cent. of oxide of silver in the salt. Another process consisted in dissolving the cyanate in nitric acid, and precipitating the silver by muriatic acid, the result was 77.05 of oxide per cent. A third analysis, made by reducing the silver of the salt, gave a result of 77.35 per cent. oxide. The mean of these is 77.3, and the theoretical number obtained by calculation is 77.23, so that the acid appears to be truly the cyanic; and the curious fact of its being the same in composition with that in the fulminating compounds of silver and mercury, but very unlike in properties, still remains undisturbed.—*Bull. Univ.*, A. viii. [illegible]8.

8. *Iodous Acid.*—According to M. Wohler, the iodous acid of

M. Sementini * is nothing more than a mixture of chloride of iodine and iodine. When saturated with carbonate of soda, the iodine in solution is precipitated, and on evaporating the solution to dryness, and heating it strongly, the residue fuses, and by proper tests is found to be a mixture of chloride and iodide of sodium.

These statements apply only to the iodous acid: as to the oxide of iodine, no source of chlorine exists in the process last described by M. Sementini.

9. *On Manganesic Acid, by* M. Unverdorben.—When manganesate of potash is distilled with a little anhydrous sulphuric acid, manganesic acid is evolved in the form of a red transparent gas, which dissolves in water, forming a red solution. The gas frequently decomposes spontaneously in the retort, with explosion, producing oxide of manganese and oxygen.

Manganesate of potash was analysed by distilling it with excess of sulphuric acid, collecting the oxygen disengaged, and estimating the proportion of protoxide of manganese and salts of potash remaining in the retort. According to these experiments the acid consists of

Manganese	.	58.74
Oxygen	.	41.26
		100.00

And the manganesate of potash of — Or being calcined

Potash	25.63	. .	32.75
Manganese acid	52.44	. .	67.25
Water	21.93		100.00
	100.00		

Ann. des Mines, 1827, p. 145.

10. *Heavy Muriatic Ether, and Hydrocarburet of Chlorine or Chloric Ether.*—Some comparative experiments have been made on these two substances by M. Vogel. He prepared the former of them by passing chlorine gas into alcohol. The muriatic acid was then separated by distilling the fluid from off chalk, in which operation the muriatic ether and alcohol passed over together, and these were divided by the addition of water, which dissolved the latter, and left the former. The chloric ether was made as usual from chlorine and olefiant gases. The results that were obtained by acting on these substances by a high temperature, potash, phosphorus, &c., induced M. Vogel to consider them as identical in composition, notwithstanding some differences in their physical properties; the specific gravity of the muriatic ether was 1.134, that of the chloric ether 1.214, and the odour of the latter is more aromatic, and the taste more sweet than of the former.

Whilst passing the chlorine into the alcohol, M. Vogel observed

* See the last volume of this Journal, p. 477.

that if the sun shone upon the substances when the action was nearly complete, each bubble of chlorine as it entered the alcohol produced a bright purple flame, a dense white vapour, and caused violent concussions in the liquid; another curious instance, in addition to the many that are known, of the power of solar light over chemical action.—*Journ. de Pharm.* 1826, p. 627.

11. *Test for the Presence of Nitric Acid.*—The following method is one devised by Dr. Liebig, for the detection of this substance, which it will effect, he says, when there is not more than a four-hundredth part of the acid present. The liquid to be examined must be mixed with sufficient sulphuric solution of indigo to acquire a distinct blue colour, a few drops of sulphuric acid added, and the whole boiled. If the liquid contains a nitrate, it will be bleached, or, if the quantity is very small, rendered yellow. By adding a little muriate of soda to the liquid before applying heat, a five-hundredth of nitric acid may easily be discovered.—*Ann. de Chimie*, xxxv. 80.

12. *Peculiar Formation of Nitre.*—The leaves and stems of beet root contain oxalate and malate of potash. Some leaves were tied together and hung up in a warm and slightly-humid place, where there was but little light, to dry. Being examined at the end of several months, they were found penetrated with, and covered by, an immense number of minute crystals of nitre. The oxalic and malic acids had been replaced by nitric acid; but whether from animalized matter naturally in the leaves of the plant, or from the action of the air, or in what manner, is not known.—M. Henri Braconnot, *Ann. de Chimie*, xxxv. 260.

13. *Experiments on Fluoric Acid and Fluates, by* M. Kuhlman.—These experiments were made with dry sulphuric acid and fluor spar, with the intention of proving that fluor spar is truly a compound of fluorine and calcium, and not of fluoric acid and oxide of calcium. A quantity of anhydrous sulphuric acid was prepared with great care, and collected in a glass tube; the latter was then connected with a platina tube charged with fluor spar, which had previously been calcined in a platina crucible, and a glass tube was connected with the other end of the platina tube for the purpose of conducting and facilitating the collection of the gas evolved over mercury. The fluor spar was heated to redness, and then the temperature of the sulphuric acid raised so as to cause a stream of it in vapour to pass over the fluor spar; but there was not the slightest reaction, the sulphuric acid recondensed in part in the farthest tube, and no trace of fluoric acid was produced. Dry sulphuric acid was then put, in the liquid state, in contact with dry fluor spar, but there was no decomposition, and no portion of the spar was con-

verted into sulphate of lime. The first experiment was then repeated, with the difference of using hydrated sulphuric acid of specific gravity 1.842, and there was instantly much fluoric acid produced, which acted upon the glass.

As Berzelius found 100 parts of fluor spar, when acted upon by sulphuric acid, to yield 175 parts of sulphate of lime, equal to 73.553 parts of lime, or 52.819 of calcium, it follows that 100 parts of fluoride of calcium should contain 47.181 of fluorine and 52.819 of calcium. By the assistance of this result, and further experiments, M. Kuhlman proceeded to ascertain the composition of hydro-fluoric acid. Dry muriatic acid gas was passed over calcined fluor spar heated to redness in a tube of platina; the fluoride of calcium was decomposed, free hydro-fluoric acid was evolved, and chloride of lime remained in the tube. The hydro-fluoric acid acted upon the glass tubes, but being received in water was entirely dissolved, with the exception of the silica it had separated from the glass: no trace of hydrogen appeared. One hundred parts of fluoride of calcium thus treated became 143.417 parts of chloride of calcium, the 52.819 parts of calcium having united to 90.598 parts of chlorine. But this latter quantity must have liberated 2.511 parts of hydrogen, which must, therefore, have combined with the 47.181 parts of fluorine in the spar, to form 49.692 parts of hydro-fluoric acid. This latter body, therefore, consists of 94.941 fluorine, and 5.059 of hydrogen per cent. A small quantity of chlorine was set at liberty during the experiment, the author thinks, from a little manganese in the fluor spar.

M. Kuhlman found that all the chlorides, when subjected to the action of anhydrous sulphuric acid in vapour, resisted decomposition, except the chloride of sodium, which gave a small quantity of sulphate of soda, and a double salt of soda and platina, crystallizing in fine needles of a yellow colour. No doubt is entertained that, in the latter case, the common salt and sulphuric acid were not perfectly dry.—*Bull. Univ.*

14. *Crystallization of Phosphorus.*—By the fusion and careful refrigeration of a large quantity of phosphorus, M. Frantween has obtained very fine crystals of an octoedral form, and as large in size as a cherry-stone.

15. *Solutions of Phosphorus in Oils.*—The solutions of phosphorus in fixed oils are so luminous as often to be resorted to for the exhibition of this peculiar property of phosphorus; but M. Walcker has remarked, that the power which they ordinarily possess is instantaneously destroyed by the addition of small quantities only of certain other substances, as the essential oils. The rectified oils of turpentine and amber, the oils of rosemary, bergamotte, lemon, camomile, angelica root, juniper berries, and parsley seed,

the oil obtained by tho distillation of the nutmeg, all produce this effect when their quantity is not more than one-fiftieth part of the luminous oily solution of phosphorus. The same effect is produced by adding about a fifth of the oils of anniseed, cajeput, lavender, rue, sassafras, fern, cascarilla, mint, orange flowers, fennel, valerian, cherry laurel, or bitter almonds, or balsam of copaiba; but the oil of cinnamon, rectified petroleum, balsam of Peru, and camphor, have no such effect.—*Annal. der Phys.* 1826, p. 125.

16. *On the Inflammation of Powder when struck by Brass, &c.*—Iron has been excluded from powder-works as subject to cause sparks by a blow, and brass aud copper have been recommended in its place. M. le Col. Aubert has remarked, that brass on brass can inflame powder, and has made experiments on the subject before a committee, the result of which is as follows:—Inflammation of the powder takes place when the blow is given by iron against iron; iron against brass; brass against brass; iron against marble; lead against lead, or against wood, when the blow is produced by a leaden hall shot from a fire-arm. As yet the powder has not been inflamed by the blow of an iron hammer against lead or wood.—*Bull. de la Soc. d'Encouragement; Bull. Univ.*

17. *Cementation of Iron by Cast Iron.*—Pure iron, when surrounded by, and in contact with, cast iron turnings, and heated, is carbonised very rapidly, so as to harden, to temper, and, in fact, to exhibit all the properties of steel. M. Gautier finds this a very advantageous process in numerous cases, especially where the articles to be case-hardened, or converted into steel, are small, as iron wire, or wire gauze. The temperature required is not so high as that necessary in the ordinary process of cementation, and the pieces to be carbonised are not injured in form. The kind of cast iron used should be the gray metal, and the more nunutely it is divided the more rapid and complete is the operation. By covering the mass of cast metal, in which the iron to be carbonised is enveloped, with sand, oxidation, from contact of the air, is prevented, and the cast metal may be used many times. Plumbago experimented with in the same manner does not produce the effect.—*Jour. de Pharmacie*, 1827, p. 18.

18. *On the Preparation of Ferro-prussiate of Potash, by* M. Gautier.—Numerous investigations induced M. Gautier to conclude, that, i. When animal matter is calcined alone it yields but little cyanogen. ii. That when mixed with potash it gives more, but the cyanuret is not ferruretted. iii. That ammonia is then produced in large quantity. iv. That the substitution of nitre for potash, and the addition of iron or scales of iron, augmented the production of cyanogen, and gave a ferro-prussiate. The following is the process of manufacture to which M. Gautier has ultimately arrived,

and which he has practised for some years. The proportions of the materials are—

Blood, considered as in the dry state,	3 parts
Nitre	1 part
Iron scales	$\frac{1}{30}$ of the blood employed.

The blood is first to be coagulated in a large copper cauldron, and the serum being separated by means of a press, the coagulum is to be returned to the cauldron with the nitre and iron. The quantity of water contained in the blood is sufficient to liquify the salt, so as to allow of an uniform mixture being effected. The mixture is then removed, and exposed in an airy situation to dry, the putrefaction of the blood being prevented by the nitre. When the desiccation is complete, the mixture is charged into cast iron cylinders, which are fixed in a reverberatory furnace, and in all things resemble those used in the preparation of animal charcoal. These are to be raised to a brown red heat, until no more vapour is disengaged, and then left until nearly cold, after which the contents are to be withdrawn and put into a wooden vat, with twelve or fifteen times their weight of water, for an hour. The fluid is then to be filtered through a cloth, and evaporated until of 32° of Beaué (specific gravity 1.284.) Being then left to cool, a large quantity of well-crystallized bi-carbonate of potash is obtained. M. Gautier says he has not, as yet, been able to explain how it is that this bi-carbonate has been formed at so high a temperature; a portion also appears to be decomposed during the evaporation of the solution, which, at first but slightly alkaline, becomes sensibly so by a prolonged evaporation.

As the same product is not obtained when potash is used in place of nitre, it is probable that the elements of the nitric acid perform a particular part in the operation.

The solution which has given the crystals of carbonate of potash contains a little carbonate of potash, and much ferro-prussiate of potash. It is to be concentrated to 34° (specific gravity 1.306),and placed in wooden vessels lined with lead. In the course of some days a greenish crystalline mass is obtained, which being redissolved in a fresh quantity of pure water, and evaporated to 32° or 33° (specific gravity 1.295), is to be recrystallized.

Sometimes, when using potash, M. Gautier has mixed nitre with it, and has always obtained a richer product than when potash alone had been employed.—*Jour. de Phar.* 1827, p. 11.

19. *Sulphocyanide of Potassium in Saliva.*—MM. Tiedemann and Gmelin have observed the existence of this peculiar compound in saliva, in two cases; the one when the fluid was secreted during smoking, and the other when no such stimulus was applied.—*Ann. de Chimie*, xxxv. 266.

20. *Decomposition of Sulphate of Copper by Tartaric Acid.*—

M. Planche has observed, that when sulphate of copper is dissolved in wine vinegar, for the purpose of preparing a corrosive liquid to be applied to corns on the feet, that the tartaric acid present in the vinegar displaces the sulphuric acid from a part of the salt, and an insoluble acid tartrate of copper is produced.

21. *Separation of Arsenic from Nickel or Cobalt.*—The following process by M. Woehler seems among the best of those intended for freeing nickel or cobalt from arsenic in the dry way. It is founded upon the circumstances that many alloys, when heated with sulphuret of potash, become changed into a mixture of sulphurets, and that sulphuret of arsenic is very soluble in sulphuret of potash. One part of kupfernickle, fused and reduced to fine powder, is to be mixed with 3 parts of carbonate of potash, and 3 parts of sulphur, in a covered Hessian crucible. The heat is to be gradually raised to redness, and until the mass is just entering into fusion, and by no means so highly as to fuse the sulphuret of nickel which is formed. When cold, water is to be added, which will dissolve the sulphuret of potash, and leave a yellow crystalline powder, which is sulphuret of nickel, retaining, perhaps, a little copper or cobalt, but no arsenic, if the operation has been well performed. When, however, the object is to have the nickel perfectly pure, it should be fused a second time with sulphur and potash.

The method of freeing cobalt from arsenic, is the same as for nickel; but it is then necessary to perform the operation a second time. The cobalt (that of Tunaberg) has never been perfectly freed from arsenic by one operation, but has never retained any after the second.—*Archiv für Bergbau*, 1826, p. 186.

22. *Compounds of Gold.*—According to late experiments of Dr. Thomson, peroxide of gold consists of

1	atom gold	25
3	„ oxygen	3
		28

and is consequently a teroxide. Muriate of gold consists of

2	atoms muriatic acid	9.25
1	„ per oxide of gold	28.
5	„ water	5.625
		42.875

Edin. Journal, p. 182.

23. *Chemical Researches relative to certain Ancient Substances.*—M. Vauquelin has analyzed, i. A poignard blade formed of copper only; ii. A mirror, which was found to consist of 85 parts of copper, 14 of tin, and 1 of iron per cent.; iii. A blue colour found in a tomb:

it was composed of silica 70 parts; lime 9; oxide of copper 15; oxide of iron 1; soda mixed with potash 4. A blue identical with this, both in colour and composition, was found in the bottom of a furnace in which copper had been fused at Romilly.

M. D'Arcet has examined a bone from the fore part of an ox, which had been placed as an offering to the divinity in an Egyptian tomb, and found that it contained as much gelatine as recent bone, although rather less is obtained by muriatic acid, (20 per cent. instead of 27) because of a deterioration of the bone. When burnt, it gave an animal black as deep in colour as that from recent bone.

M. Le Baillif has examined some grains of corn, which were so well preserved, that when put into boiling water iodine produced the blue colour dependent upon starch. He also made some experiments on a gummy substance, and on two cords from a musical instrument; the latter were of animal substance.

M. Raspail examined some grain which was supposed to be wheat, but found it to be torrified barley; it was covered with a substance communicated probably by the oil and incense with which the grains were bathed when consecrated. Similar grains were obtained by roasting common barley.

The account of most of these researches is given in the Catalogue raisonné et historique des Antiquités découvertes en Egypte, by M. Passalacqua.—*Bull. Univ.* A. vii. 264.

24. *On the Bitter Substance produced by the action of Nitric Acid on Indigo, Silk, and Aloes, by* M. Just Liebeg.—The process by which M. Liebeg obtains a pure and uniform substance from the action of nitric acid on indigo, is as follows:—A portion of the best indigo is to be broken into small fragments, and moderately heated with eight or ten times its weight of nitric acid of moderate strength. It will dissolve, evolving an abundance of nitrous vapours and swelling up in the vessel. After the scum has fallen, the liquid is to be boiled, and nitric acid added, whilst any disengagement of red vapours is occasioned by it. When the liquid has become cold, a large quantity of semi-transparent yellow crystals will be formed, and if the operation has been well conducted, no artificial tannin or resin will be obtained. The crystals are to be washed with cold water, and then boiled in water sufficient to dissolve them. If any oily drops of tannin form on the surface of the solution, they must be carefully removed by touching them with filtering paper. Then filtering the fluid, and allowing it to cool, yellow brilliant crystalline plates will be obtained, which will not lose their lustre by washing.

To obtain the substance perfectly pure, the crystals must be redissolved in boiling water, and neutralized by carbonate of potash. Upon cooling, a salt of potash will crystallize, which should be purified by repeated crystallizations.

On mixing the first mother liquor with water, a considerable brown precipitate will be obtained, which being dissolved in boiling

water, and neutralized by carbonate of potash, will furnish a large quantity of the potash salt. All the potash salt obtained in these operations is to be re-dissolved in boiling water, and nitric, muriatic, or sulphuric acid added; as the solution cools, the peculiar substance will be observed to form very brilliant plates of a clear yellow colour, generally in equilateral triangular forms.

Sometimes crystals are not formed after the action of the nitric acid on the indigo, in which case the liquor must be evaporated, and water added, when the substance will precipitate, and must be purified as already described. Four parts of indigo yield one of the pure substance.

When the substance is heated, it fuses, and is volatilized without decomposition; when subjected to a sudden strong heat, it inflames without explosion, its vapours burning with a yellow flame, and a carbonaceous residue remaining. It is but little soluble in cold water, but much more in boiling water; the solution has a bright yellow colour, reddens litmus, has an extremely bitter taste, and acts like a strong acid on metallic oxides, dissolving them, and forming peculiar crystallizable salts.—Ether and alcohol dissolve the substance readily.

When fused in chlorine or with iodine, it is not decomposed, nor does solution of chlorine affect it. Cold sulphuric acid has no action on it; when hot, it dissolves it, but water separates the substance without alteration. Boiling muriatic acid does not affect it, and nitro-muriatic acid only with great difficulty.

These results show that no nitric acid is present in the substance, and other experiments prove that no oxide of nitrogen exists in it; it contains no oxalic or other organic acid, for when its salt is boiled with chloride of gold, the latter is not reduced.

When heated to redness with oxide of copper, it gave a mixture of nitrogen and carbonic acid, in the exact proportion of 1 volume of the former, to 5 of the latter. This was a constant result, and in no case was any sulphuric or muriatic acid left in the copper. 0.0625 grammes of the substance thus decomposed, gave 45 cubic centimeters of the mixed gases, estimated at 0° C. (32° F.) and the pressure of 28 inches of mercury, according to which the acid would be composed of carbon 32.392; nitrogen 15.2144; oxygen 52.3936 per cent. From the mean of several experiments, it appeared that the following might represent the composition correctly.—

$12\frac{1}{2}$ atoms of carbon	. . .	93.75	or	31.5128
$2\frac{1}{2}$ „ azote	. . . .	43.75	„	14.7060
16 „ oxygen	. . .	160.00	„	53.7812
		297.5		100.

100 parts of the acid neutralize a quantity of base equivalent to 3.26 of oxygen, which is to the oxygen of the acid, as 1 : 16; the equivalent number of the acid derived from the analysis of the

barytic salt was 306.3; by adding only $\frac{1}{4}$ per cent. to the quantity of baryta obtained in the experiment, 297.5, or the number expressed by the above formula, would be obtained.

When a salt of potash or baryta was decomposed by oxide of copper and heat, the quantity of carbonic acid produced was a little short of five times the quantity of nitrogen; but, upon adding that retained by the alkali or earth, the proportion became exactly the same as in the former cases.

Welter's bitter principle was prepared by acting on silk with ten or twelve times its weight of nitric acid. The liquid, slightly coloured at first, acquired a deep yellow upon adding water. It was neutralized by carbonate of potash whilst hot, and left to cool, and the salt of potash thus obtained, decomposed by muriatic, nitric, or sulphuric acid. This acid, crystallized like that from indigo, formed the same salts, and was composed in the same manner. Silk furnishes much less of the substance than indigo. Dr. Liebeg has called this substance *carbazolic acid.* The most important salts formed by it have the following properties:—

Carbazotate of Potash—crystallizes in long yellow quadrilateral needles, semi-transparent and very brilliant; it dissolves in 260 parts of water at 59° F., and in much less, boiling water: a saturated boiling solution becomes a yellow mass of needles, from which scarcely any fluid will run. Strong acids decompose it; yet when an alcoholic solution of carbazotic acid is added to a solution of nitre, crystallized carbazotate of potash, after some time, precipitates.—Alcohol does not dissolve it. When a little is gradually heated in a glass tube, it first fuses, and then suddenly explodes, breaking the tube to atoms; traces of charcoal are observed on the fragments. This salt precipitates a solution of the protonitrate of mercury, but not salts, containing the peroxide, or those of copper, lead, cobalt, iron, lime, baryta, strontia, or magnesia. The slight solubility of this salt supplies an easy method of testing and separating potash in a fluid. Even the potash in tincture of litmus may be discovered by it; for, on adding a few drops of carbazotic acid, dissolved in alcohol, to infusion of litmus, crystals of the salt gradually separated. The saturated solution of the salt at 50° F., is not troubled by muriate of platina. The salt contains no water of crystallization. It was analyzed by converting a portion of it into chloride of potassium by muriatic acid: its composition is,—

Carbazotic acid	83.79
Potash	16.21
	100.00

Carbazotate of Soda—crystallizes in fine silky yellow needles, having the general properties of the salt of potash, but soluble in from 20 to 24 parts of water, at 59° F.

Carbazotate of Ammonia forms very long, flattened, brilliant,

yellow crystals, very soluble in water. Heated carefully in a glass tube, it fuses, and is volatilized without decomposition; heated suddenly, it inflames without explosion, and leaves much carbonaceous residue.

Carbazotate of Baryta, obtained by heating carbonate of baryta, and carbazotic acid with water. It crystallizes in quadrangular prisms of a deep colour, and dissolves easily in water. When heated, it fuses, and is decomposed with very powerful explosion, producing a vivid yellow flame. The explosion is as powerful as that of fulminating silver; a solution of chloride of potassium to which carbazotate of baryta has been added, produces a precipitate of the potash salt, and not more than $1\frac{1}{3}$ per cent. of potash remains in solution. 100 parts of the crystallized salt contain,—

Carbazotic acid . .	69.16	oxygen of the acid	.	16
Baryta	21.60	„ earth	.	1
Water	9.24	„ water	.	8
	100.00			

Carbazotate of Lime, obtained like the salt of baryta, forms flattened quadrangular prisms, very soluble in water, and detonating like the salt of potash.

Carbazotate of Magnesia forms very long indistinct needles, of a clear yellow colour; is very soluble, and detonates violently.

Carbazotate of Copper, prepared by decomposing sulphate of copper by carbazotate of baryta: it crystallizes with difficulty, the crystals being of a fine green colour; it is deliquescent; when heated, it is decomposed without explosion, and even without inflammation.

Carbazotate of Silver.—Carbazotic acid readily dissolves oxide of silver, when heated with it and water; and the solution, gradually evaporated, yields starry groups of fine acicular crystals of the colour and lustre of gold; the salt dissolves readily in water; when heated to a certain degree, it does not detonate, but fuses like gunpowder.

Proto-Carbazotate of Mercury, obtained in small yellow triangular crystals, by mixing boiling solutions of the carbazotate of potash or soda, and proto-nitrate of mercury. It requires more than 1200 parts of water for its solution: for its perfect purification, it should be heated with a solution of chloride of potassium, the insoluble portion separated whilst the liquid is lost, and the peculiar salt allowed to deposit as the temperature falls. When heated, it behaves like the salt of silver.

All these salts detonate much more powerfully when heated in close vessels, than when heated in the air; and it was a curious thing to observe, that those with bases yielding oxygen most readily, were those which exploded with least force. By heating some of the salts previously mixed with chloride of potassium, &c., to retard the action, it appeared that no carbonic oxide, but only car-

bonic acid and azote were evolved during their decomposition by heat.

On the Bitter Principle from Aloes.—Upon distilling 8 parts of nitric acid from 1 part of the extract of aloes, and adding water to the remaining fluid, a resinous reddish yellow substance precipitated, which, by washing, became pulverulent—it was discovered by M. Braconnot. Upon evaporating the liquid separated from the precipitate, it gave large yellow rhomboidal crystals, not transparent, and but slightly soluble. These crystals, at first mistaken for a particular substance, were soon found to be a combination of oxalic acid with the bitter of aloes. The bitter substances of aloes dissolved in 800 parts of water, at 59° F., but in a smaller quantity of boiling water. This solution has a superb purple colour. Silk boiled in it acquired a very fine purple colour, on which neither soap nor acids effected any change, except nitric acid; this changed the colour to yellow, but it was restored simply by washing in water. All shades may be given to this colour by proper mordants. Wool is dyed black in a peculiarly beautiful manner, by the same process, and light has no influence on the colour. Leather acquires a purple colour; cotton, a rose colour; but the latter will not resist soap. Dr. Liebeg thinks that this is the only substance from which a permanent rose dye for silk may be expected.—*Ann. de Chimie*, xxxv. 72.

25. *On the Existence of Crystals of Oxalate of Lime in Plants.*—M. Raspail has read a memoir to the Academy of Sciences, to prove the analogy which exists in arrangement between the crystals of silica, which are found in sponges, and those of oxalate of lime occurring in the tissue of phanerogamous plants.

The latter crystals were observed, for the first time, by Rafn and Jurine, who regarded them as organs of which they knew not the use. They were then observed by M. de Candolle, who called them *raphides*, and gave a figure of them, which, however, is inaccurate. These crystals are really very regular tetraedrons. In many plants, as *orchis, pandanus, ornithogalum, jacinthus, phytolaca decandria, mesembryanthemum deltoides*, &c. they are very small, not being more than $\frac{1}{200}$ of a millimetre (.0002 of an inch) in width, and $\frac{1}{10}$ (.004 of an inch) in length. But, in the tubercles of the Florence iris, they are as much as $\frac{1}{50}$ (.0008 of an inch) in width, and $\frac{1}{3}$ (.01312 of an inch) in length, so as to be easily capable of examination.—*Bull. Univ.* B. xi. 376.

26. *Fallacy of Infusion of Litmus as a Test, by* M. Magnus.—When pure water is heated for a sufficient time with infusion of litmus, reddened by an acid, it restores the blue colour. It is supposed that the heat gradually causes the free sulphuric acid, which had occasioned the reddening, to combine with the excess of alkali contained in the infusion, and thus to cause the restoration of the blue colour. Hence this preparation cannot be used to test the

presence of ammonia in a solution, as water alone produces the effect anticipated from the alkali. The earthy salts contained in ordinary water also produce this effect.—*Jour. de Pharmacie.*

27. *Tests for the Natural Colouring Matter of Wine.*—M. A. Chevalier states,—i. That potash may be employed as a re-agent, to ascertain the natural colour of wines, which it changes from red to a bottle green, or brownish green—ii. That the change of colour produced by this substance upon wine is different for wine of different ages—iii. That no precipitation of the colouring matter takes place, the latter remaining dissolved by the potash—iv. That the acetate of lead should not be employed as a test of the colour of wines, because it is capable of producing various colours with wines of a natural colour only—v. That the same is the case with lime-water, with muriate of tin mixed with ammonia, and with subacetate of lead—vi. That ammonia may be employed for this purpose, the changes of colour which it produces not perceptibly varying—vii. That the same is the case with a solution of alum to which a certain quantity of potash has been added, and which may, therefore, be used for the purpose.—*Annales de l'Industrie.*

28. *Test of the Presence of Opium.*—Dr. Hare says he can detect opium in solution, when the quantity is not more than that given, by adding ten drops of laudanum to half a gallon of water. The following is the process:—a few drops of solution of acetate of lead is to be added to the solution containing the drug; after some time an observable quantity of meconiate of lead will fall down: from six to twelve hours may sometimes be required, and the precipitation is best effected in a conical glass vessel, for then, by gentle stirring now and then to liberate that which adheres to the side, the insoluble salt may be collected together at the bottom. About thirty drops of sulphuric acid are then to be poured on to the meconiate by means of a glass tube, after which as much of a solution of red sulphate of iron is to be added in the same manner. The sulphuric acid will liberate the meconic acid, and thus enable it to produce with the iron the appropriate colour, which demonstrates the presence of that acid, and consequently of opium.—*Silliman's Journal*, xii. 290.

29. *Denarcotized Laudanum.*—Thinking it important to ascertain whether, by the removal of narcotine from opium, the unpleasant effects which, according to the opinions at present entertained upon that subject, are produced by that drug would be removed, Dr. Hare prepared some opium with ether, guided by Robiquet's statement that narcotine was soluble in that fluid: the opium was shaved by rubbing it on the face of a jack-plane, and subjected four times successively to as much ether of the specific gravity 0.735 as would cover it, the operation being performed in a small Papin's digester, at a temperature near the boiling point of ether, and each

portion of the fluid being allowed twenty-four hours for its action. A crystalline deposition was soon observed in the ether which had been removed from the opium, and, allowing the stopper of the vessel to remain out, nearly the whole of the liquid evaporated in a few days, and left much coloured crystalline matter. This, Dr. Hare has no doubt, was narcotine in an impure state. The opium was afterwards subjected to as much alcohol as would have been required to convert it into laudanum, had it been in the ordinary state; and this being administered medicinally, was found to occasion none of those uneasy and unpleasant sensations which often follow the use of ordinary opium.—*Silliman's Journal*, xii. 291.

30. *Extraction of Morphia from Dry Poppy Heads*, by M. Tilloy.—Make an aqueous extract of the heads, add alcohol to the extract, separate the alcoholic solution, and distil it; by this means the gummy matter is separated. An extract like syrup will be obtained by the distillation, which, being heated to make it thinner, and of the consistency of treacle, is to be again treated with alcohol; a separation of more gum, with much nitrate of potash, will be effected. The solution being withdrawn, is to be distilled, and the extract which will remain is to be acted upon by a sufficient quantity of water, and filtered, to separate the resinous matter present. The morphia may then be separated from this liquid, either by ammonia, carbonate of soda, or magnesia. Ammonia does not precipitate all the morphia; carbonate of soda precipitates a large quantity, but it separates resinous matter also, which is found mingled with the morphia. Magnesia is preferable; but as the liquid contains much free acetic acid, it is expensive to employ the necessary quantity of pure magnesia: the liquid may, therefore, be partly saturated, whilst hot, by carbonate of magnesia, or even by carbonate of lime. A judgment, when no more must be added, must be formed from the effervescence; then pure magnesia is to be added, which will cause the liberation of ammonia; the whole is to be left for twenty-four hours to cool: being then filtered, the precipitate is to be washed, and, when dry, acted upon by alcohol. Operating in this manner, morphia may be obtained from all kinds of poppies.—*Bull. Univ.* E. viii. 10.

31. *Preparation of Morphia.*—Some curious experiments have been described to the Académie de Médecine, by M. Robinet, relative to the preparation of morphia. Having operated on the residue of opium by muriatic acid, and precipitated the morphia from the muriatic solution by lime, he wished to ascertain whether the mother liquor contained any morphia that had escaped precipitation. He, therefore, passed a current of carbonic acid gas through the solution, to precipitate the lime in excess: this precipitate being washed, dried, and acted upon by alcohol, was found mixed with a very large proportion of morphia, which could

be thus separated. The washings of the precipitate being examined, were found free from morphia.

M. Henry observed, at the same time, that, from experiments made at La Pharmacie Centrale, it appeared that much more morphia was obtained in those processes in which lime had been used to precipitate the morphia, than in those in which magnesia had been used.—*Bull. Univ.* C. xi. 225.

32. *Easy Method of obtaining Meconic Acid*, by Dr. Hare.—If to an aqueous infusion of opium we add subacetate of lead, a copious precipitation of meconiate of lead ensues: this being collected by a filter, and exposed to sulphuretted hydrogen, meconic acid is liberated: the solution is of a reddish amber colour, and furnishes, by evaporation, crystals of the same hue. A very small quantity produces a very striking effect in reddening solution of peroxide of iron. Instead of sulphuretted hydrogen, sulphuric acid may be used to liberate the meconic acid: the presence of the former in excess does not seem to interfere with the power of reddening ferruginous solutions, but any excess of sulphuric acid may be removed by whitening, which is not acted upon sensibly by meconic acid. Yet, the acid procured in this way did not crystallize so handsomely, or with so much facility, as that obtained by sulphuretted hydrogen.

33. *On a New Vegetable Acid.*—This acid is crystallizable, but the forms have not as yet been determined: it is less soluble in cold water than tartaric acid; its aqueous solution precipitates lime water in white floculi, just like tartaric acid, but the precipitate, if dissolved in muriatic acid, re-appears on adding ammonia, whilst that produced by tartaric acid does not produce this effect. The new acid has a greater affinity for lime than muriatic or nitric acids, for it precipitates the muriate and nitrate of this earth in the manner of oxalic acid, but it differs from the latter in not precipitating a solution of sulphate of lime. With potash it forms an acid salt, slightly soluble in cold water: it precipitates acetate of lead, and the precipitate holds much water in combination: the tartrate of lead, on the contrary, is anhydrous. Notwithstanding these circumstances, the equivalent number of this acid is within a few thousandths of that of tartaric acid: when distilled, it is decomposed, and produces an acid yellow liquid like tartaric acid, leaving a light charcoal burning without residuum. M. Gay Lussac is engaged in developing the chemical history of this substance.—*Bull. Univ.* A. vii. 327.

34. *Altheine, a new Vegetable Principle.*—M. Bacon gives the following directions for the preparation of this substance, which he has discovered in the *Althea officinalis*. An extract of the roots of the plant is to be made by means of cold water, and, when concentrated,

acted upon by boiling alcohol: the latter will dissolve the acid malate of altheine, oil, &c.: the different alcoholic decoctions are to be put together and will throw down a crystalline deposite as they cool; the latter is to be separated and dissolved in water, and the solution, when filtered, is to be evaporated by a moderate heat, until like a syrup, and then set aside to crystallize. The crystals procured are to be washed with a small quantity of pure water, to separate the yellow matter from them, and then dried upon paper. These crystals appear, to the naked eye, like grains, needles, and feathers, but under the microscope present a hexaedral form. They are of a fine emerald green colour, transparent, brilliant, inodorous; unaltered in the air; they redden litmus paper, are soluble in water, and insoluble in alcohol. The aquenus solution of these crystals, acted upon by cold magnesia and filtered, then restores the colour of reddened litmus paper; renders syrup of violets green; and when evaporated furnishes the altheine free from malic acid. When thus pure, the substance crystallizes in regular hexaedral forms or in rhomboidal octnedrons; it affects litmus and violets as just described: it is transparent, of an emerald green colour, brilliant, inodorous, slightly sapid, unaltered by air, very soluble in water, not soluble in alcohol, soluble in acetic acid, with which it forms a crystalline salt.—*Ann. de Chimie*, xxxiv. 201.

35. *Rheine, a new Substance from Rhubarb.*—By acting upon one part of Chinese rhubarb with 8 parts of nitric acid, s. g. 1.32, at a moderate temperature, reducing the whole to the consistence of syrup, and then diffusing it through water, M. Vaudin obtained a precipitate which possessed peculiar characters, and to which he gave the name of *Rheine*. When dry, it is of an orange yellow colour, without any particular odour, and slightly bitter. It dissolves in water as well as in alcohol and ether: the solutions become yellow by acids, and rose red by alkalis. It burns nearly in the manuer of amadou. Rhubarb acted upon by ether only gave a similar substance, a circumstance which proves that Rheine exists ready formed in rhubarb, and that it is not acted upon by nitric acid.—*Ann. de Chimie*, xxxiv. 192.

36. *On Dragon's Blood, and a new Substance which it contains, by* M. Melandri.—Pure dragon's blood is, according to M. Melandri, a scarce substance; the drops in which it occurs are rarely transparent, generally opaque, and with a rough fracture: its colour is blood red. Besides being soluble in alcohol it is entirely soluble in oil and also in hot water, though a large quantity of the latter fluid is required for the purpose. The aqueous solutinn is bitter, astringent, and of a fine purple colour; by cooling, it becomes milky and red. Gelatine does not alter its appearance; a proof that the substance contains no tannin. Sulphate of iron forms a pale reddish precipitate, so that no evidence of gallic acid is afforded.

Supposing that this substance might contain a principle analogous to that latterly observed by M. Pelletier in logwood, &c. a portion of it was dissolved in strong alcohol, the solution evaporated until very concentrated, and then poured into cold water, an agglomerated spongy substance was precipitated, which, after being washed with cold water and filtered, was triturated with water containing $\frac{1}{100}$th of sulphuric acid, and exhibited traces of chemical action at a temperature of 22° (61°.6 F.) It then deposited a substance upon the sides of the vessel, and the liquid became yellow and very acid. The sediment, being carefully washed with water, was of a fine red colour, varying according to the state of aggregation; it had no taste or smell; was flexible between the fingers, and was quite fluid at 55° (131° F.). This substance, which the author has called *Dracine,* has some analogy with the vegeto-alkalis, although its affinity for acids is but slight. The sulphate may be obtained, he says, by adding sulphuric acid diluted with alcohol to an alcoholic solution of *dracine,* precipitating the mixture by cold water, and then applying a little heat; the sulphate of dracine collects at the bottom, is to be washed with cold water until the latter no longer reddens litmus paper, and then dissolved in hot water. This solution becomes red by the smallest quantity of alkalis, and may be used as a very sensible test of their presence. Dracine is also a good test for acids, assuming a yellow colour with them. The small quantity of carbonate of lime in filtering paper may be detected by sulphate of dracine, the yellow solution instantly becoming red from its action, and thus showing its presence. —*Bull. Univ.* C. xi. p. 157.

37. *Purification of Madder, by the Separation of its Yellow Colouring Matter.*—The experiments of MM. Kuhlman, Colin, and Robiquet *, have induced M. G. H. de Kurrer to publish the means which he has resorted to for the purification of madder, by the separation of the yellow colouring matter from it; and thus rendering it more fit to supply the various red, lilac, violet, and brown colours which are required upon wool, silk, cotton and linen. Three tubs or vessels are placed by the side of each other: in summer they may be in the open air under shelter, but in the winter should be placed in an airy cellar where the temperature may be retained at 18° or 20° R. (73° to 77° F.). The first is that in which the soaking and fermentation is to be effected: it should be 2 feet 8 inches deep, and 2 feet 6 inches in diameter, for from fifty to fifty-five pounds of madder. The second, or washing vessel, should be $5\frac{1}{2}$ feet deep, and 3 feet in diameter; it should have three wooden cocks fixed into it, the first 2 feet, the second 3 feet, and the third 4 feet from the bottom. The third tub is for deposition; its height should be $4\frac{1}{2}$ feet, and it should have a cock at $1\frac{1}{2}$ foot from the bottom.

* See page 239 of the last volume.

On commencing the operation, 50 or 55 lb. of pulverised madder are to be put into the first vessel, water is to be added, and stirred into the mass until it stands $1\frac{1}{2}$ inch above the madder. The whole is then to be left until fermentation comes on and has formed a coat of madder at the surface; this usually takes place in 36 hours, and at latest in 48 hours, according to the temperature. The mass should now be transferred into the second vessel, which is then to be filled with water, and being left for two hours, the madder will fall to the bottom. The upper cock is then to be opened, after that the second, and then the third; and the water which runs from the two latter is to be put into the third vessel, that the rest of the madder may separate from it. The madder in the second vessel is then to be washed a second, third, or fourth time until the washing water is colourless. Thus purified, the madder may be used in the processes of dyeing, according to the known methods; but it is important in summer that it should be used immediately, that a new (the vinous) fermentation may be avoided. The madder deposited in the third vessel, when washed and deposited, may be used like the rest. The liquid first separated after the fermentation may be used in the preparation of hot indigo baths, &c. instead of madder.—*Bull. Univ.* P. vii. 352.

38. *On Indigo and Indigogene, by M. Liebeg.*—$1\frac{1}{2}$ part of pure indigo, 2 parts of proto-sulphate of iron, $2\frac{1}{2}$ parts of hydrate of lime, and from 50 to 60 parts of water, were digested together for 24 hours in a close vessel, which had previously been filled with hydrogen. The clear liquor over the sulphate of lime and oxide of iron, had a yellowish red colour, and was separated by a syphon filled with hydrogen, and mixed with diluted muriatic acid, containing some sulphite of ammonia dissolved; a dense white precipitate was formed, becoming blue in the air. This was gathered in a filter without contact of air, and washed with boiled water containing sulphite of ammonia in solution, and dried at 212°, in close vessels, through which a current of hydrogen was continually passed. The upper surface of the mass became of a blue colour, but the lower remained of a dull white.

This white substance was called Indigogene. It did not change colour in dry air, but under water became of a deep blue, which by drying, assumed a coppery appearance. The blue substance volatilized by heat without leaving any residue, forming purple vapours, which condensed, when cold, into crystals differing in nothing from sublimed indigo. *Indigogene* dissolves in alkalis without neutralizing them: it is also soluble in alcohol, but insoluble in water or acids.

A given quantity of this indigogene was acted upon by ammonia, and the weight of the undissolved blue portion ascertained, it appeared that the weight of the pure portion dissolved was 0.404 grammes (6.224 grains.) The solution was put into an inverted

jar, over mercury, and oxygen gas gradually passed in until absorption ceased, and then the liquid containing the precipitated indigo was evaporated to dryness at 212°. The weight of the substance was increased to 0.047, *i. e.* 11.5 per cent.

Not having obtained indigogene *perfectly* pure, M. Liebeg did not attempt to analyze it for the ultimate composition. He remarks, that indigo is, perhaps, the only organic body from which one of its constituent parts may be taken without total decomposition; and which, by oxidation, passes to the state of an indifferent body, having much analogy with peroxides.—*Ann. de Chimie*, xxxv. 269.

39. *On the mutual Action of Ethers, and other Substances.*—From experiments made by M. Henry, he concludes that when metals easily oxidizable, or oxides which unite with acetic acid, are put into sulphuric ether, they produce larger or smaller quantities of acetates, probably, not by decomposing the sulphuric ether, but the acetic ether which is always mixed with it; and that it is in consequence of the saturation of the acetic acid set free from the ether by this decomposition, that sulphuric ether does not redden litmus paper when evaporated, whereas it acts differently when being slightly heated, the quantity of acetic ether contained in it is allowed to decompose by the action of the air.

Nitric and acetic ethers are described as being easily decomposed by the action of many bodies without the assistance of heat, if aided by time. Amongst the products of the action are the acids of the ethers, acetates, and alcohol which dissolves the salts formed. —*Jour. de Chimie Méd.*

40. *Faraday's Chemical Manipulation.*—The kindness of a friend at Bristol has pointed out to me an error in the directions relative to alkalimetry, which I have given in the above work: this I am desirous of correcting, and, by permission of Mr. Brande, have the opportunity of doing so in the *Quarterly Journal of Science.*

The mistake, which arose from using the wrong specific gravity of two that were required in calculation, occurs in the paragraphs (599, 600,) but fortunately is prevented from occasioning any experimental error by the directions given in (602). The acid of specific gravity, 1.141, directed to be used, is too strong for the quantities marked upon the tube. The substitution of one of specific gravity 1.127, will correct the error, and may be obtained very nearly by mixing 19 parts, by weight, of strong oil of vitriol, with 81 parts of water.

The alterations required may be made in the volume with a pen, as for errors of the press, by reading "1.127" for "1.141" in lines 25 and 30 of page 276, and lines 2 and 13 of page 277; and "nineteen" for "one" in line 27, and "eighty-one" for "four" in line 28 of page 276.—M. F.

III. Natural History.

1. *On the Supposed Influence of the Moon,* by M. Arago.—There is an impression very general with gardeners, that the moon has a particular effect on plants, especially in certain months. The gardeners near Paris gave the name of the *lune rousse* to the moon, which, beginning in April, becomes full either at the end of the month, or more generally in May. According to them the light of the moon, in the months of April and May, injures the shoots of plants, and that, when the sky is clear, the leaves and buds exposed to this light become red or brown, and are killed, though the thermometer in the atmosphere is several degrees above the freezing point: they confirm this observation, by remarking that, when the rays of the moon are stopped in consequence of the existence of clouds in the air, that then the plants are not injured, although the temperature and other circumstances are the same.

M. Arago explains this observation of practical men, by a reference to the facts and principles established by Dr. Wells. He has shown that, in a clear night, exposed bodies may frequently have their temperatures reduced below that of the surrounding atmosphere, solely by the effect of radiation, the difference being as much as 6, 7, 10, or more degrees, but that it does not take place when the heavens are obscured. M. Arago then observes, that the temperature is often not more than 4, 5, or 6 degrees above the freezing point during the nights of April and May, and that when the night is clear, consequently when the moon is bright, the temperature of the leaves and buds may often be brought by radiation below the freezing point, whilst the air remains above it, and consequently an effect be produced, which, though not dependent upon, accompanies the brilliant unobscured state of the moon—the absence of these injurious effects, when the moon is obscured, being also as perfectly accounted for by these principles, from the knowledge that the same clouds which obscure the moon will prevent the radiation of heat from the plants. Hence, as M. Arago observes, the observation of the gardener is correct as far as it goes, though the interpretation of the effect which he generally gives is incorrect.—*Annuaire du Bureau des Long.* 1827, p. 162.

2. *Luminous Appearances in the Atmosphere.*—An account is given at page 242 of our last volume, from Silliman's Journal, of certain spots in the air near the horizon, which have been seen highly luminous in Ohio, United States, by Mr. Atwater, and which often induce the supposition that fires exist in their direction. Mr. Webster says—"I have observed similar phenomena in New England: I recollect one instance, when I resided at Amherst, in Hampshire County, Mass., a bright light in the north-east, near

the horizon, appeared as the light of a building on fire appears at night, at the distance of several miles. I expected, in that instance, every hour to hear that some building in Shutesbury, or New Salem, had been burnt, and, so strong was my belief of it, that I repeatedly asked my neighbours whether they had heard of any such event. At last, I met a gentleman who had just come from one of those towns, who told me he had heard of no fire from that quarter, which convinced me the phenomenon was merely atmospheric."—*Silliman's Journal*, xii. 380.

3. *On the Determination of the Mean Temperature of the Air.*—This subject has been investigated by M. G. G. Hallstrœm, who gives the following algebraic formula, which correctly represents the mean temperature for all Europe.

$$v = \tfrac{1}{2}(xf + xe) - 0.33 + 0.41 \text{ sin. } [(n - 1)\, 30^\circ + 124^\circ\, 8')]$$

v = mean temperature.

n = the ordinal number of the month for which the temperature is to be calculated (thus, for March, $n = 3$).

$\frac{1}{2}(xf + xe)$ = the mean temperature taken as the mean of observations taken at ten o'clock in the morning and evening.

In winter $\frac{1}{2}(xf + xe) = v$ very nearly; whilst, in summer, this quantity is $\frac{3}{4}$ of a degree greater than v at Paris, Halle, and Abo.—*Annal. der Phys. und Chem.* 1825, p. 373.

4. *Indelible Writing.*—As the art of man can unmake whatever the art of man can make, we have no right to expect an *indelible ink*: however, a sort of approximation to it may be made as follows:—Let a saturated solution of indigo and madder in boiling water be made, in such proportions as give a purple tint; add to it from one sixth to one eighth of its weight of sulphuric acid, according to the thickness and strength of the paper to be used: this makes an ink which flows pretty freely from the pen, and when writing, which has been executed with it, is exposed to a considerable, but gradual, heat from the fire, it becomes completely black, the letters being burnt in and charred by the action of the sulphuric acid. *If the acid has not been used in sufficient quantity to destroy the texture of the paper, and reduce it to the state of tinder, the colour may be discharged by the oxymuriatic and oxalic acids, and their compounds, though not without great difficulty.* When the full proportion of acid has been employed, a little crumpling and rubbing of the paper reduces the carbonaceous matter of the letters to powder; but by putting a black ground behind them, they may be preserved, and thus a species of *indelible writing* is procured, (for the letters are, in a manner, stamped out of the paper,) which might be useful for some purposes, perhaps for the signature of bank-notes.

5. *Peculiar Crystals of Quartz.*—Mr. W. Phillips has met with some remarkable crystals of quartz, which occurred imbedded in the

limestone of the Black-rock, near Cork. They are from the fourth to the half of an inch in length, and about half their length in width: they are smooth, externally, for the most part, and sometimes considerably bright; they are of the colour termed smoky, or brown quartz, externally, and may easily be separated from the limestone, leaving a cavity of their exact form. On trying to cleave them, they yielded parallel to one or other of the planes of the pyramid, like common quartz, but at such fractures appeared to consist of alternate and concentric prisms of smoky transparent quartz, and of gray opaque, and somewhat granular limestone. On applying muriatic acid to the surface, effervescence occurred along the gray parts, proving the presence of limestone, but soon ceased: after an action continued for some weeks, the gray parts became cellular, and so soft, as to admit of being scraped by a knife. Mr. Phillips says, it seems reasonable to conclude that such part of the gray substance as does not yield to the action of the acid is siliceous or quartzose; and that the prime difference between it and the smoky quartz surrounding it consists in the different circumstances of crystalline aggregation under which they are deposited. The crystals, with the somewhat analogous case of the Fontainebleau sandstone, may serve to assist in the illustration of some points relative to the laws of affinity, as operating in the formation of crystals.—*Phil. Mag.* N. S., ii. 123.

6. *Native Iron not Meteoric.*—The following notice is by Mr. C. A. Lee. Native iron, on Canaan mountain, a mile and a half from the South Meetinghouse (Conn. U. S.). This is particularly interesting, as it is the first instance in which native iron, not meteoric, has been found in America. It was discovered by Major Barrall, of Canaan, while employed in surveying, many years ago. It formed a thin stratum, or plate, in a mass of mica slate, which seemed to have been broken from an adjoining ledge. It presents the usual characters of native iron, and is easily malleable. For some distance around the place where it was found the needle will not traverse, and a great proportion of the tallest trees have been struck with lightning. Whether these phenomena are connected with the existence of a large mass of native iron, I leave for others to determine: the facts, however, may be relied on.

The specimen has been examined chemically, by Mr. Shepherd, at Yale College. It is invested with highly crystalline plumbago, and splits by the intervention of plates of plumbago into pyramidal and tetrahedral masses. It is not equal to meteoric iron in malleability, toughness, and flexibility, and has not the silvery white appearance of that iron. Its specific gravity is from 5.95 to 6.72. It has native steel intermingled in it, but contains no nickel, or any other alloy.

Major Barrall has only been to the place where this iron occurred once, and no other person has ever been to the place, or knows where it is.—*Silliman's Journal*, xii. 154.

7. *Native Argentiferous Gold.*—M. Boussingault, who has had the opportunity of examining numerous specimens of argentiferous native gold from the Columbian mines, thinks that they are atomic; he has found 1 atom of silver united to 2, 3, 5, 6, and 8 atoms of gold, and considers it probable that the other combinations to complete the series may occur. He has assumed 24.86 as the number for gold, and 27.03 as the number for silver. The following are some of the experimental results:—

Native Gold of Marmato.—Pale yellow octoedral crystals:

Gold . .	73.45	3 atoms .	73.40
Silver . .	26.48	1 " .	26.60
Loss . .	00.07		

Native Gold of Titiribi:

Gold. . .	74.00	3 atoms .	73.40
Silver . . .	26.00	1 " .	26.60

Native Gold of Malpaso.—Yellow irregular flattened grains:

Gold . .	88.24	8 atoms .	88.04
Silver . .	11.76	1 " .	11.96

Native Gold of Rio-Sucio.—Deep-coloured large irregular grains:

Gold . .	87.94	8 atoms .	88.04
Silver . .	12.06	1 " .	11.96

Native Gold of the Otra Mina.—Pale yellow octoedral crystals:

Gold . .	73.4	3 atoms .	73.40
Silver . .	26.6	1 " .	26.60

Native Gold of Guamo.—Brass-yellow indeterminate crystals:

Gold . .	73.68	3 atoms .	73.40
Silver . .	26.32	1 " .	26.60

Native Gold of Llano.—Small flattened grains—reddish:

Gold . .	88.58	8 atoms .	88.04
Silver . .	11.42	1 " .	11.96

Native Gold of Baja.—Porous:

Gold . .	88.15	8 atoms .	88.04
Silver . .	11.85	1 " .	11.96

Native Gold of Ojas-Anchas.—Yellowish red plates:

Gold . .	84.5	6 atoms .	84.71
Silver . . .	15.5	1 " .	15.29

Native Gold of Trinidad, near Santa Rosa de Osos.—A solid piece of 50 grains:

Gold . .	82.4	4 atoms .	82.14
Silver . .	17.6	1 " .	17.86

Native Gold of Transylvania (Europe).—Pale yellow cubic crystals:

Gold . .	64.52	2 atoms .	64.77
Silver . .	35.48	1 " .	35.23

Native Gold of Santa Rosa de Osos.—A mass weighing 710 grains:

Gold . .	64.93	2 atoms .	64.77
Silver . .	35.07	1 " .	35.23

M. Boussingault has remarked a singlar deficiency in the

specific gravity of the native alloys of gold and silver when compared with calculation, or with the results obtained from an alloy similar in composition prepared by fusion; thus the native gold of Marmato has a specific gravity of 12.666, whereas, by calculation, it ought to be 16.931. The gold of Malpaso, by experiment, is 14.706, by calculation, 18.223, and by fusion, 18.1. The gold of Santa Rosa, by experiment, is 14.149, and by calculation, 16.175. This difference, M. Boussingault says, is not due to porosity in the native gold, as he has observed it in the granular and fine varieties, but a peculiar character of the metal in this state. Such an enormous difference, however, is one that can be admitted only upon repeated experimental proofs, made in the most unexceptionable manner; and, considering that it is only in some of the metals that any permanent difference in specific gravity can be established, and even with them to but a small extent, would be a fact so important as to be worth extreme trouble in the verification.—*Annales de Chimie*, xxxiv. 408.

8. *Prothéeïte—a new Mineral.*—This mineral was discovered in 1826, at Rothenkoph, in the valley of Zillerthal, Tyrol. It occurs in rectangular prisms, generally without distinct summits, and rough at both ends. The angles are very seldom truncated, the faces are striated longitudinally. The crystals are of various sizes, some being very small, but they have occurred 5 inches in length, and two in width; the longitudinal fracture is lamellar, the cross-fracture conchoidal. The substance is usually fissured, nearly opaque in large specimens, translucent or diaphanous in small masses. Its colour is crysolite green or white, or between the two; its lustre between that of glass and the diamond; it is heavy; a good conductor of heat; hard enough to scratch glass; infusible before the blowpipe; highly electric by friction. The white crystals have a fibrous texture, which, as well as the colour, seems the result of decomposition. When cut and polished, the mineral assumes a great variety of aspects; the green parts then resemble the finest crysolites, but the fibrous white parts, when cut of a round form, present one or two reflections on a transparent ground which move as the stone is moved, just like those from the cat's eye; these reflections are very brilliant, and are accompanied by numerous iris colours, which move like those of the opal. This phenomenon is often observed in the rough stone, which, when exposed to light, exhibit certain deep red tints of a cupreous colour, and metallic lustre on all the faces.—*Bull. Univ.* B. xi. 42.

9. *Volcanic Bisulphuret of Copper.*—M. N. Covelli, during his examinations of Mount Vesuvius, has observed some particular actions going on, especially in the fumeroles on the eastern side of the mountain, and within the crater. Speaking of the former, he says, "Here there are fumeroles in which pure chloride of lead

sublimes into white and yellow crystallizations, which fusing in the hotter places form nacres, gum, and stalactites. In many parts the sulphuretted hydrogen, evolved within the fumeroles, reacts on the chloride, and forms sulphuret of lead, dispersed in small scales through the scoria. Other fumeroles produce very thin scales of the black oxide of copper; these are very brilliant, metalloidal, and flexible, and are produced by the action of the vapour of water at a red heat on the chloride of copper, which may be observed on disturbing the fumeroles. Here and there the reaction of aqueous vapour on the perchloride of iron produces metalloidal scales of the peroxide of iron; whilst further on, the same vapour, acting on mixtures of the two chlorides, produces oligiste iron in small crystals, aggregated on the scoria. The muriatic acid resulting from these actions, and the sulphuric acid which is formed by the decomposition of hydrosulphurets and sulphates, attack the iron, lime, copper, alumine, potash, &c., in the lavas and scoria, and hence result a number of other productions which line the passages of the fumeroles.

M. Covelli descended into the crater, until within 300 feet of the edge of the large eastern opening, from which the great current of lava flowed in 1822. Here the fumeroles presented the most beautiful crystallizations of sulphate of lime and sulphur. On examining the scoria they were found incrusted and covered with a substance, having all the shades of colour belonging to blue, green, and black. Sometimes it resembled a spider's web in appearance, sometimes soot deposited in the cavities of the scoria. Many specimens were collected, and also a portion of water condensed from the vapours which issued forth, and which evidently contained sulphuretted hydrogen and muriatic acid. The temperature of the vapour was as high as 85° C., in some places, and even up to 90°, at half a foot beneath the surface.

The water being examined was found to contain only a little sulphuretted hydrogen, and a little muriatic acid. The black substance was soon ascertained to be a pure sulphuret of copper. Being analyzed, 100 parts yielded 32 parts of sulphur, and 66 of copper, a loss of two parts being incurred, which accords very nearly with the composition of the bi-sulphuret of copper. The blue and bluish-green substances were found to be mixtures of this sulphuret with sulphate and hydro-sulphuret of copper.

M. Covelli concludes that this substance has been formed by the action of sulphuretted hydrogen on the sulphate and muriate of copper evolved by these fumeroles; and observes, that its composition accords with such an opinion, the deutoxide being that which forms the Vesuvian cupreous salts.—*Ann. de Chimie*, xxxv. 105.

10. *Fall of the Lake Souwando in Russia.*—This lake, situated in the parish of Sakkola, in the Russian government of Wibourg, and surrounded by the lands of the Barons Friedrichs, was near

40 versts in length, and had the form of a Γ, or Greek G. Before the year 1818, it was separated from the lake of Ladoga by an interval about a verst in width, called Taipale, on which was a sandy hill; its waters flowed into the river Wuoxa, which united the lakes of Saima and Ladoga. On the 14th May, 1818, the waters of the lake Souwando, increased by the thaw and the tempests, overcame the natural dyke at the foot of the lake, threw down the hill of sand, rapidly flowed into the lower lake, carrying away all the surrounding grounds, and for ever destroyed the barrier which had previously separated them. A chapel and a countryman's house were carried away with the pastures and meadows; the waters of the lower lake were much disturbed, and the surface covered with ruins. The level of the lake Souwando fell 12½ archines, and its length is now only 15 versts. Its waters no longer flow off by the Wuoxa, but pass into the lower lake by several falls through a deep canal. The land which has been uncovered by the water is already cultivated, and the beauty of the surrounding country said to be increased.—*Bull. Univ.*, F. x. 133.

11. *Vegetable Torpor observed in the Roots of the Black Mulberry-tree.*—A very old mulberry-tree was broken into four quarters by the wind in 1790. Two of the quarters were destroyed, the other two remained growing for a few years, but the last of them was removed in 1802. An elder-tree grew in the place of the mulberry-tree, without doubt from berries which had fallen into the middle of the old trunk of the latter. This elder-tree died in 1826, and at the time of its languishing about a dozen of mulberry shoots started forth to the day. M. Dureau de la Malle ascertained that these did not spring from seeds, but from the roots of the old mulberry-tree, which had thus lain in the ground in an apparently inactive state, for 24 years, to send forth shoots at last.—*Ann. de Sciences Nat.* ix. 338.

12. *Method of increasing the Odour of Roses.*—For this purpose, according to the author of the method, a large onion is to be planted by the side of the rose tree in such a manner that it shall touch the foot of the latter. The roses which will be produced will have an odour much stronger and more agreeable than such as have not been thus treated, and the water distilled from these roses is equally superior to that prepared by means of ordinary rose leaves.—*Œkonom. Neuigk.*;—*Bull. Univ.*

13. *Pine Apples.*—A great improvement may be made in keeping pine apples by twisting off their crowns, which are generally suffered to remain and to live upon the fruit till they have sucked out all the goodness. It will be very easy for fruiterers to keep a few crowns by them in water, which can be pegged or stuck on with dough, for show, when the fruit is served up, or artificial ones

may be made. A pine apple will keep for a long time when its crown is removed, and will also be greatly improved in flavour, for the more aqueous parts of the fruit gradually evaporate, and leave it much more saccharine and vinous in its flavour; which natural process is totally destroyed by the vegetation of the crown, just upon the same principle that an onion or carrot loses its flavour when it begins to sprout in the spring.

14. *Mode of Condensing and Preserving Vegetable Substances for Ships' Provision, &c.*—The quantity of liquid matter which enters into the constitution of vegetables is very great; when they are deprived of it their bulk is very trifling. That preparation of animal food called *pemmican*, in which six pounds of meat are condensed into the space of one, is mainly effected by abstracting all the fluid from it. Vegetables may be treated in the same way: let them undergo the process of boiling over a fierce wood fire, so as to preserve their colour when *completely* cooked; grind them into a complete pulp by some such means as are used to crush apples for cider, &c.; then let them be subjected to the action of the press, (being first put into hair bags, or treated as grapes are in wine countries,) till all the fluid matter is separated from them; the remainder of their substance becomes wonderfully condensed, and as hard as the *marc* from the wine press. Then let it be rammed hard into carefully glazed air-tight jars, (or tin cases, if preferred,) and subjected to the Appertian process for preserving animal and vegetable matters, (well known, by-the-by, to our grandmothers, who preserved gooseberries in this way from time immemorial.) If jars are used, they may be sufficiently secured by having two pieces of bladder tied successively over them; when the air within is absorbed by heating the inclosed substance, their surface becomes concave by the pressure of the atmosphere, and as long as it remains in this state the matter within is safe. If it should be thought requisite to preserve the flavour of the vegetables entire, an extract should be made from the expressed liquid, and added to the *marc*. But spinage, cabbage, and many others, have abundance of flavour in them in their dry state without this addition. The preparation of the vegetable matter for use is accomplished by adding a sufficient quantity of milk, water, gravy, lime juice, &c., to the *marc*, and warming it up. Let the government, and the dealers in ships' provision, look to this; a sufficient quantity of this *vegetable pemmican* would be the greatest luxury to a ship's crew, and render the scurvy utterly obsolete. It is worthy of remark, that the most irritable stomach is not offended by vegetables treated in this way.

15. *Rewards for the Discovery of Quinia, and for Lithotrity.*—The Académie des Sciences has adjudged a prize of 10,000 francs to MM. Pelletier and Caventou, for their discovery and introduction

into use of sulphate of quinia; and another prize of 10,000 francs to M. Civiale, for having been the first to practise lithotrity on the living body, and for having successfully operated by his method on a great number of persons afflicted with the stone in the bladder.

16. *Upon the Gaseous Exhalations of the Skin.*—M. Collard de Martigny, having experimented on this subject, has obtained results which tend to reconcile the differences existing between previous observers. The Count de Milly first announced, in the year 1777, that an aëriform fluid escapes in great quantity from the surface of the skin, and he considered the gas as carbonic acid. Cruikshank, Jurene, and Abernethy participated in this opinion. Ingenhouz, on the other hand, maintained that the air so secreted was azote. M. Frousset adopted the opinion of Ingenhouz, and endeavoured to confirm it by experiments. Lastly, Priestley and Fontana questioned the reality of a gaseous exhalation from the skin; and Fourcroy positively denied it.

From the experiments of M. Collard de Martigny, he deduces,

i. That a gaseous exhalation really takes place from the skin.

ii. This exhalation is not morbid: it is observable in health.

iii. It is composed of carbonic acid and azote, in very variable proportions. The following experiment was frequently made. The bubbles of air which are disengaged from the skin were received into a funnel, the top of which was closed: they were then passed into a graduated tube, and agitated with a solution of potash. The height to which the solution rose in the tube indicated the quantities of carbonic acid that had been absorbed. All these operations were made at the same temperature and pressure. Neither hydrogen nor oxygen gas were discovered in this air.

iv. It does occur continually; but very often we may vainly attempt to discover it, which has been the cause of error in the results of Priestley, Fontana and Fourcroy. It is especially suspended after exercise long continued in the middle of the day, or immediately after taking an abundant meal. Sometimes it is suspended without any apparent cause.

v. The quantity also is very variable; but it was observed to be constantly in an inverse ratio to the cutaneous absorption.

vi. The proportions of the two gases vary very much, and sometimes the exhaled gas consists almost entirely of azote: in other instances the predominance of carbonic acid is so great that it appears to be the only product.—*Med. Rep.*, N. S. v. 75.

17. *Effects of Galvanism in Cases of Asphyxia by submersion.*—M. Leroy d'Etioles has addressed a letter to the Académie de Médecine, in reply to an assertion made by M. Thillaye respecting the inutility of galvanism in cases of asphyxia. The former says, that when a short and fine needle is inserted in the sides of the body between the eighth and ninth ribs, so as to come in contact with

the attachment of the diaphragm, and then the current of electricity from 25 or 30 pair of inch plates passed through them, that the diaphragm immediately contracts, and an inspiration is effected. Upon breaking the communication, and again completing it, a second inspiration is occasioned, and by continuing these means, a regular respiration may ultimately be occasioned. This power thus applied has always succeeded with him in experiments on drowned animals.—*Bull. Univ.*, C. xi. 213.

18. *Recovery from Drowning.*—M. Bourgeois had occasion accidentally to give assistance in a case where, after a person had been twenty minutes under water, he was taken out, and by a very common but serious mistake, carried with his head downwards. The usual means were tried unremittingly, but unsuccessfully, for a whole hour, but at the end of that time a little blood flowed from a vein that had been opened, and a ligature being placed on the arm, ten ounces of blood were withdrawn: the circulation and respiration were then gradually re-established, horrible convulsions, and a frightful state of tetanus coming on at the same time; copious bleeding was again effected, after which a propensity to sleep came on: a third bleeding the following morning was followed by the recovery of the patient. Hence M. Bourgeois concludes that the means of recovering a drowned person should never be abandoned until the decomposition of the body has commenced.—*Bull. Univ.*, C. xi. 213.

19. *Preservation of Cantharides.*—It is stated by M. Farines that the active part of cantharides exists only in the soft organs of the insect; that these are the parts which are attacked by a species of acarus, and that in this way the cantharides are injured. Camphor has no power of preventing the attacks of the acarus; but M. Farines believes that pyroligneous acid will be found effectual, and proposes to prepare cantharides with it, and even to kill them at the time when they are collected by submersion in it.

20. *Chloride of Lime in cases of Burns.*—The good effect of chloride of lime in cases of burns is confirmed by the experience of M. Lisfranc. He has applied it in many cases of that kind, sometimes immediately after the accident, sometimes after the application of emollient cataplasms. Lint is moistened in a solution more or less strong of chloride of lime, and then applied to the place, being covered over with waxed cloth. The cure has been singularly hastened under its influence; and in one case where almost the whole of the lower limbs, the arms and face, had been burnt, the use of the chloride recovered the patient from the stupor into which he had fallen at the end of four days, and a perfect recovery was effected two months after the accident.—*Bull. Univ.*, C. xi. 77.

21. *Cure of Nasal Polypi.*—Dr. Primus of Babenhausen asserts, that the saffronised tincture of opium (of the Prussian Pharmacopœia) possesses the property of gradually destroying nasal polypi when applied to them. Certain cures, which have been thus effected, have already been published, and a striking one occurred in January, 1826. A man, 46 years of age, had one in each nostril. The tincture was applied several times a day to the bases of the polypi, by means of a small hair-brush or lint roll. In eight days the tumours had assumed a paler appearance, and lost a little in volume; a serous secretion from the nose, which had existed for a long time, was diminished, and the pituitary membrane had acquired a more lively tint, as if in a sub-inflammatory state. The application was continued, the tumours continued to decrease, and at the end of three weeks had entirely disappeared.—*Mediz. Chirurg. Zeitung*, 1826, p. 13.

22. *Bite of the Viper.*—M. Jacopo Sacchi, of Barzio in Valsasina, having had occasion to take charge of some cases in which injury had been inflicted by the bite of a viper (Coluber Berus), transferred his observations upon them into the hands of Professor Paletta. From these it appears that ammonia, recommended by Dr. Mangili, in 1813, although an excellent remedy in many cases, is by no means sufficient in all, but must occasionally be seconded by every possible means. Although sometimes nature alone has power sufficient to overcome the bite of a viper, yet, at other times, the injury is so great and sudden as to resemble the effects of hydrocyanic acid. In these cases he recommends that the patient should be put into a hot bed covered with woollen clothes, and the most powerful sudorifics with some tonics administered internally. Friction should be applied all over the body, and at the same time the wounds are to be enlarged, cupping-glasses applied, and tow, dipped in ammonia, applied to the spot.

23. *Experiments on the Poison of the Viper.*—M. Desaulx confirms the fact that dogs can swallow with impunity even large quantities of the poison of vipers. He observed also that when this poison was withdrawn from the vesicles it soon lost in power, and after a certain time became inert: a portion ten days old being introduced into a fresh wound of a living animal, only caused slight tumefaction on the part. Mangili, on the contrary, found it, when hermetically sealed up, to retain its virulence for many months. The species of viper from which M. Desaulx obtained his poison is not mentioned. —*Bull. Univ.* C. xi. 142.

24. *Destruction of Moles.*—The following method of destroying moles is asserted, by the Count de Boisseulh, to be excellent. Grounds much infested by these animals have been perfectly freed from them by means of it. A number of worms must be procured, killed, and powdered with pulverised vomica-nut; the whole is to

be mixed and left for twenty-four hours. The mole-tracks are then to be opened, and two or three of these worms placed in each hole. If the meadow is large, they cannot be placed in every hole; but by multiplying them as much as possible, a good result is sure to be obtained.—*Ann. de Agricul. de la Charente.*

25. *On growing Salad-herbs at Sea.*—On long sea-voyages, whatever esculent roots, or fruit, or whatever vegetable essences may be stowed in the steward's stores, whether for the use of the officers or crew, nothing can be a greater treat to the former, especially within the tropics, than a dish of fresh salubrious salad-herbs. The want of such an addition to the ordinary fare on board a ship has often been a cause of disease, and misfortune, and even death! —it is needless, therefore, to insist on the usefulness, or to state the antiscorbutic, and consequently sanatory qualities, of fresh vegetables in such situations; and however limited the means to supply such a want as is described below, yet, as it may be highly useful to convalescents, and in individual cases, the publication may not be deemed altogether valueless.

Provide one, two, or three deal boards, made of well-seasoned inch stuff, sixteen inches square, with a ledge all round, rising one inch above the smooth surface of the board; and as it is intended to hold water, the ledges must be closely and neatly fitted: at each corner a nail, or small hook, should be placed, with strings tied into a loop above, by which the board may be slung in the necessary horizontal position; a thin covering-board, made of the same material and dimensions, is also necessary, and which will serve for all the boards.

Pieces of the *thickest* flannel must be had for each board, cut so as to fit exactly within the ledges. These flannels require to be well soaked, and repeatedly washed in boiling water, before they can be used, to discharge from them whatever is pernicious to vegetation as they come from the manufacturer's hands.

The board and flannel thus prepared, dip the flannel in water, and place on the boards; sow the seeds pretty thick and regularly; sprinkle them lightly with the hand, till all are moistened and the flannel completely saturated; in which state it should always be kept during the growth of the plants. Too much water floats the seeds when first put on, and are thereby shifted from their places by the motion of the ship. The cover-board must now be put on, and the whole hung up in its place. The use of this board is to assist the vegetation of the seeds, which it will do sufficiently in the course of twenty-four hours; after which it may be laid aside.

The board must be frequently examined, and when the moisture thereon is diminished by evaporation, or imbibed by the crop, a supply must be given, just enough to keep the flannel in the proper saturated state.

In six or seven days the crop will be (if the weather has been favourable) two inches high,—it is then fit for use. The produce

of one board yields about as much as will fill a middle-sized salad-bowl, and when dressed up with the usual condiments of onion, salt, vinegar, and oil, a most agreeable salad will be composed, and a most acceptable treat to the guests at the captain's table.

It is necessary that the board, as well as the flannel, be scalded, well washed, and dried in the sun, before it can be used again;—and as one board yields one crop per week, two, or even three boards may be used at the same time, in order to secure a regular supply. Larger boards are not so convenient, because they can only be hung in some by-corner of a cabin, quarter-gallery, or state-room; where they may not only be out of the way, but out of the sun and currents of air.

The herbs suitable to be raised in this way are, radish, mustard, and common garden-cress. The two first answer best within the tropics; the last does not, being too delicate and diminutive;—but this does very well when the ship is no nearer the equator than thirty degrees of latitude. One peck of radish, another of mustard, and two quarts of cress, will be sufficient for an India and China voyage, a supply of which may be had in China. I. M.

26. *Chinese Method of fattening Fish.*—The Chinese are celebrated for their commercial acumen, indefatigable industry, and natural adroitness,—in making the most of every gift of nature bestowed on their fertile country. Useful as well as ornamental vegetables engross their every care; and animals which are the most profitably reared, and which yield the greatest quantity of rich and savoury food, are preferred by them for supplying their larders and stews. Their *hortus dietetica* would form a considerable list; and though they do not use such a variety of butcher's meat and fowl as Europeans do, yet in the articles of pork, geese, and ducks, they surpass, in the use of fish they equal, us, and in their domestication and management of them they excel all other nations.

A few observations on their *piscinas*, or fish-stews, is the design of this paper; not merely as an historical description, but as an object for imitation in this or any other country.

For twenty or thirty miles round Canton, and as far as the eye can reach on each side of the river on which that city stands, the general face of the country appears nearly a level plain, with but little undulation of surface. The level is, however, richly studded with beautiful hills, which diversify the landscape, and seem to rise out of the plain so abruptly, that they form the most picturesque features, united with the most pleasing combinations. The soil of the plain consists of a pure alluvial earth of great fertility and depth, and very retentive of water; which, by the by, is a proof that, notwithstanding their claim to high chronological antiquity, the waters of the deluge remained much longer (perhaps for ages) on this portion of the continent of Asia, than it did in the interior: and the circumstance of many of their hills being cultivated to the

very top, their numerous water-plants, and their almost amphibious habits as to their domiciles, are still further proofs that the country was, once, more of an aquaium than it now is. Hence the facility of making canals, which are their high-roads (as wheel-carriages, and beasts of draught, are too expensive appendages, for the systematic economy of the celestial empire!) and hence the ease with which a pond may be made in any otherwise useless corner. Such tanks, or ponds, are generally met with in market-garden grounds, where they serve the double purpose of a reservoir, and a stew for rearing and fattening fish.

When a pond is made for this purpose, and filled with water, the owner goes to market, and buys as many young store-fish as his pond can conveniently hold; this he can easily do, as almost all their fish are brought to market alive. Placed in the stew, they are regularly fed morning and evening, or as often as the feeder finds it necessary; their food is chiefly boiled rice, to which is added, the blood of any animals they may kill, wash from their stewing-pots and dishes, &c., indeed any animal offal or vegetable matter which the fish will eat. It is said, they also use some oleaceous medicament in the food, to make the fish more voracious, in order to accelerate their fattening; but of this the writer could obtain no authentic account.

Fish so fed and treated, advance in size rapidly, though not to any great weight; as the kind (a species of perch) which came under observation, never arrive at much more than a pound avoirdupois; but from the length of three or four inches, when first put in, they grow to eight or nine in a few months, and are then marketable. Drafts from the pond are then occasionally made; the largest are first taken off, and conveyed in large shallow tubs of water to market: if sold, well; if not, they are brought back and replaced in the stew, until they can be disposed of.

This business of fish-feeding is so managed that the stock are all fattened off about the time the water is most wanted for the garden-crops. The pond is then cleaned out, the mud carefully saved, or spread as manure,—again filled with water, stocked with young fry, and fed as before.

An intelligent Chinaman, from whom the writer had the above detail, and who showed him as much of the process as could be seen during a residence of three months, declared as his belief, that a spot of ground, containing from twenty to thirty square yards, would yield a greater annual profit as a stew, than it would in any other way to which it could possibly be applied.

That fish may be tamed, suffer themselves to be caressed, and even raised out of their natural element by the hand, has been long known to naturalists; witness the famous [illegible] formerly in the pond of some religious house [illegible], with many other instances on record. But it is probable no people has carried the art of stew-feeding fish, and practising it as a profitable concern, to such lengths, as is done by the Chinese at this day. I. M.

METEOROLOGICAL DIARY for the Months of June, July, and August, 1827, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For JUNE, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Friday ...	1	[illegible]	65	29.50	29.50	SW	SW
Saturday ...	2	[illegible]	62.5	29.60	29.43	S	WbS
Sunday ...	3	[illegible]	63	29.67	29.67	W	W
Monday ...	4	44	[illegible]	29.70	29.79	W	W
Tuesday ...	5	[illegible]	69	29.70	29.59	W	W
Wednesday	6	47	58	29.47	29.60	W	NW
Thursday ...	7	43	60	29.78	29.88	W	NW
Friday ...	8	36	63	30.09	30.07	NW	W
Saturday ...	9	45	72	30.13	30.17	W	W
Sunday ...	10	48	68.5	30.17	30.10	NE	NE
Monday ...	11	45	70.5	30.09	30.09	NE	NE
Tuesday ...	12	46	66	30.02	30.02	NE	NE
Wednesday	13	51	65	30.02	29.94	NE	NE
Thursday ...	14	48	65	29.69	29.78	NE	NE
Friday ...	15	51	60	29.70	29.60	NE	NE
Saturday ...	16	52	71	[illegible]	29.57	NE	S
Sunday ...	17	54	73	[illegible]	29.56	WSW	W
Monday ...	18	54	72.5	29.78	29.79	W	W
Tuesday ...	19	50	68	29.79	29.69	W	W
Wednesday	20	48	65	29.65	29.67	W	WbS
Thursday ...	21	44	65.5	29.70	29.72	SW	W
Friday ...	22	48	64	29.80	29.88	W	W
Saturday ...	23	[illegible]	63	29.90	29.94	W	W
Sunday ...	24	43	65	29.94	29.97	W	W
Monday ...	25	40	66	29.97	29.99	WNW	NW
Tuesday ...	26	46	69	29.90	29.86	W	WbS
Wednesday	27	53.5	67	29.80	29.69	SW	SW
Thursday ...	28	56	66	29.48	29.46	SW	SW
Friday ...	29	56	70	29.46	29.53	SW	SW
Saturday ...	30	52	72	29.59	29.69	W	W

For JULY, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Sunday ...	1	55	69	29.60	29.65	E	SW
Monday ...	2	49	66.5	29.70	29.60	S	SW
Tuesday ...	3	49	69	29.63	29.73	WbS	WbS
Wednesday	4	48	73	29.99	29.99	W	WbS
Thursday ...	5	58	67	30.06	30.20	NE	NE
Friday ...	6	42	70	30.23	30.27	E	WNW
Saturday ...	7	55	75	30.27	30.25	WNW	WNW
Sunday ...	8	53	74	30.26	30.21	WbN	WbN
Monday ...	9	54	73.5	30.19	30.08	W	W
Tuesday ...	10	54	72	30.02	29.97	W	WbN
Wednesday	11	55.5	67	29.90	29.98	NW	W
Thursday ...	12	45	68	30.02	30.04	W	ENE
Friday ...	13	44	73	30.06	30.01	E	E
Saturday ...	14	46.5	71.5	30.04	[illegible]	ESE	E
Sunday ...	15	45	71	29.98	29.91	E	NE
Monday ...	16	47	71.5	29.91	29.90	E	E
Tuesday ...	17	46.5	77	29.90	29.88	SE	WbS
Wednesday	18	57	72	29.83	29.87	WSW	W
Thursday ...	19	51	68	29.87	29.71	W	SW
Friday ...	20	57	69	29.59	29.62	W	W
Saturday ...	21	50	69	29.77	29.80	W	W
Sunday ...	22	45	64	29.82	29.80	ESE	SE
Monday ...	23	57	73	29.83	29.88	EbS	W
Tuesday ...	24	58	75	29.90	29.89	W	WbS
Wednesday	25	60	72	29.82	29.82	SW	WNW
Thursday ...	26	46	69.5	29.92	29.73	W	SSW
Friday ...	27	54	74	29.90	30.00	W	W
Saturday ...	28	58	79	30.00	30.02	W	W
Sunday ...	29	54	78	30.04	29.92	ESE	SE
Monday ...	30	65	75	29.69	29.80	SSE	W
Tuesday ...	31	52	72.5	30.03	30.10	W	W

For AUGUST, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Wednesday.	1	51	72	30.04	29.95	W	
Thursday ...	2	46	77.5	29.82	29.67	W	
Friday ...	3	56	74.5	29.60	29.30	SW	W
Saturday ...	4	58	71	29.48	29.63	SW	W
Sunday ...	5	53	66	29.91	30.06	W	NE
Monday ...	6	51	67.5	30.13	30.18	NE	ENE
Tuesday ...	7	42	70	30.16	30.10	E	E
Wednesday.	8	40	68	30.04	29.98	E	E
Thursday ...	9	41	70.5	29.93	29.80	EbN	SE
Friday ...	10	57	70	29.60	29.48	SW	W
Saturday ...	11	47	66	29.48	29.44	W	W
Sunday ...	12	50	62	29.59	29.67	WSW	WNW
Monday ...	13	46	67	29.76	29.73	W	WbS
Tuesday ...	14	51	70	29.53	29.48	SW	SW
Wednesday.	15	60	68	[illegible]	29.25	SE	WbS
Thursday ...	16	51	67	29.20	29.36	S	NE
Friday ...	17	56	68	29.60	29.76	NE	NE
Saturday ...	18	54	60	29.87	29.87	NE	NE
Sunday ...	19	48	65	29.89	29.90	NE	NE
Monday ...	20	42.5	59.5	29.94	29.90	NE	NW
Tuesday ...	21	56	67.5	29.90	29.95	NE	NE
Wednesday	22	51	59	30.03	30.10	N	NW
Thursday ...	23	48	58	30.29	30.20	NW	NW
Friday ...	24	59	[illegible]	30.14	30.08	WNW	WbN
Saturday ...	25	49	60	30.00	30.04	WNW	W
Sunday ...	26		58	30.03	30.08	NW	N
Monday ...	27	[illegible]	67	30.13	30.16	N	NW
Tuesday ...	28	[illegible]	64	30.16	30.16	W	NW
Wednesday ...	29	50	63	30.24	30.24	NNE	NW
Thursday ...	30	47	67	30.18	30.07	W	W
Friday ...	31	59	63	30.12	30.20	N	NE

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*On the Means generally used with the Intention of curing a Stoop.**

When the chest and the head fall forward, the most common method of trying to correct the stoop is to put on some instrument by which the shoulders and the head are held back. To operate upon the shoulders, the common back-collar is applied, and to hold back the head, a riband is brought over the forehead and fastened to the collar.

While these instruments are kept on, the figure looks straight, though stiff and constrained; but the moment they are taken off, both the head and the shoulders fall more forward, than before their application. Many examples of the bad effect of artificially supporting the head might be offered. The following, although observed in the figure of a horse, is very demonstrative. When the rein (called the bearing-rein), by which the head of a carriage-horse is reared up, with the intention of giving him a showy figure, is loosened, the head immediately falls forward, and the neck, instead of preserving the fine arch that is so much admired, droops between the shoulders. Looking to this effect, we should at first be inclined to condemn the practice followed by horse-dealers, of reining up the head of a young horse in the stable, by means of the apparatus called a dumb-jockey. But on examining

* For this, and some other communications upon the same subject, we are chiefly indebted to our much-lamented friend and correspondent, the late Mr. Shaw, Surgeon to the Middlesex Hospital.

into this mode of fixing the head, it will be found to operate on a different principle from the bearing-rein. Instead of a simple bit, such as the horse in harness can lean his head upon, without suffering pain, a bit, calculated to tease and fret, is put into the young horse's mouth. To relieve himself from the irritation produced by this, and which is increased by the constant pull of the elastic piece of iron to which the rein is fastened, he curls up his neck, and thus brings all the muscles of the back of the neck into strong action, instead of allowing their power to be superseded by the artificial support afforded by the bearing-rein to the horse in harness *.

Many different contrivances, but all acting nearly on the same principle as the *bearing-rein*, have been proposed as means for obliging a girl to keep her head erect.

There is one mode which, to a person ignorant of anatomy, seems to be particularly well adapted for this purpose; but it is, in fact, more objectionable than the plan of tying the head back with a riband. A piece of lead, of some pounds weight,

* When the Russians wish to give a horse high action in trotting, they accustom him, while young, to wear heavy shoes on the fore feet. The resistance to be overcome necessarily increases the strength of certain muscles; and hence, when shoes of the common size are put on, the horse lifts his feet higher than one which has not been subjected to this discipline. Some opera dancers practise with lead weights on

is slung over the back in such a way that it must be supported by a riband put around the head.

Although this contrivance prevents the head for a time from falling forwards, its bad effects may be demonstrated. When the weight is on, the muscles of the back of the spine are passive, while these on the fore-part of the neck are necessarily brought into action to prevent the head from being pulled too far back: this is easily proved; for if we put the fingers on the sternal portions of the sterno-cleido muscle, which, with the small muscles on the fore-part of the throat, pull the head forwards, we shall feel them tense and in action. The increased activity of the muscles on the fore part, and the passive condition of those of the back, may be further exemplified by raising the weight when the girl is not aware of our doing so; the head will then be immediately poked forwards.

We have many opportunities of observing the incorrectness of the principle on which all similar plans for the cure of a stoop have been founded. For instance, porters who carry burthens on the back, by the assistance of a band round the forehead, always stoop; while those who carry baskets before them suspended by a band round the back of the neck, are peculiarly erect. But the most remarkable example of the effect of the head being pulled back by a weight hung behind, is the condition of the women who carry salt in the streets of Edinburgh, for they may be recognised as much by their miserable Sardonic grin, which is caused by the constant excitement of the platysma myoides muscle, as by their stoop.

Very annoying and even distressing consequences may ensue from any system of treatment where a constant resistance to the muscles of the fore-part of the neck is kept up. A gentleman had for many years worn one of the collars invented by Mr. Chesher; after some time, the muscles of the back became so weak, as to be incapable of supporting the column, while those on the fore-part of the neck were so disproportionately increased in strength, by the constant resistanee opposed to them by the strap passing from the suspending rod under the chin, that whenever the strap was loosened, the chin was forcibly drawn towards the chest. As the muscles of the back part of the neck did not offer any counteracting resistance, the

windpipe was now pressed down, or almost doubled on itself. As soon as this took place (and it was almost immediate on the attempt to sit up without the collar,) the patient was seized with such a sense of suffocation, as to be obliged to throw himself on his back. As he was able to breathe with ease as he lay on his back, his advisers were led to believe that it was the weight of the head which pressed down the windpipe. To counteract this pressure, various contrivances had been proposed to support the head. Indeed, the patient himself was so convinced, from what he had heard, that it was the weight of the head which pressed down the windpipe, and so alarmed had he become from the certainty of having a fit of suffocation when the head was left unsupported, that there was much difficulty in persuading him to believe that if the head could be made *heavier*, the sense of suffocation would be relieved. He was at length induced, although with great dread of the consequence, to allow about fourteen pounds of shot to be placed on the top of his head. He was very much alarmed, but it was highly gratifying to witness his surprise and pleasure in finding that, instead of his head being weighed down, he could support it, and could breathe with ease while in the upright posture. The following is the principle on which this plan was proposed:—the muscles of the back part of the neck had been brought into such a state, that their ordinary stimulus was not sufficient to excite them to the action necessary to counteract the efforts of those on the fore-part of the neck, which had been evidently increased in strength. The placing a weight on a certain spot on the head formed an additional stimulus to the muscles of the back part of the neck; a fact which the reader may prove by an experiment on himself.

By proceeding on this principle, by combining a variety of exercises, and by gradually diminishing the weight carried on the head, this gentleman was soon able to walk and sit in a state of great comfort, without being obliged to use any artificial support.

It is well known, that the neck-collars support almost the whole weight of the head and shoulders by the strap which passes under the chin. It must also have been observed, that the wearer very frequently pushes down the head against the

chin strap. In this way, the muscles on the fore-part necessarily become stronger, while those of the back, being deprived of their natural stimulus to action, in consequence of the rod superseding their office, become diminished in power. Even were there no change in the degree of strength in the muscles on the fore-part, the head would naturally fall, if the support afforded by the chin strap were removed; but as these muscles are increased in power, while those of the back are diminished, the head must not only fall, but even be pulled down.

However, although the collars and the lead weight, as they are generally used, are not only inefficacious, but even hurtful, they may occasionally be useful in keeping the head in a certain position, after it has been brought to it by such exercises as tend to strengthen those muscles of the back which support the shoulders and head. But the opinions commonly entertained, as to the means of counteracting an habitual stoop, are so erroneous, that even the position of a tailor sitting on his shopboard is better than the plans generally recommended. This at first appears ridiculous; but the manner a tailor holds his body when he walks, proves that there is something in his habits which tends to the correction of a stoop; for he is quite a caricature of a strutting erect figure, especially in the way he bends in his loins and carries his head.

The peculiarity of the tailor's gait proceeds, in a certain degree, from the bent position in which he sits: but this explanation is not at first satisfactory, since it may be observed that other tradesmen, who also stoop while at work, generally have their head inclined forwards, and have also a distinct and habitual bend in the neck; such, especially, is the condition of persons who sit at a table and stoop forwards, as watchmakers, engravers, &c. It is not difficult to explain the cause of the difference, and the inquiry will assist in directing us to the principles which we ought to recollect in our operations upon the spine.

In the sitting position of the tailor, the head hangs so low, and so complete an arch is formed between it and the pelvis, that the muscles of the spine are called into strong action to support the head; the necessary consequence of this is, that these muscles become even unnaturally strong, or at least so strong as to predominate over those by which the spine is

pulled forward. But the bent position is not the only cause of increase in the strength of the muscles, for it depends also on the exercise given by frequently jerking the head backwards. In those who stoop from the middle of the body, as in writing or working at a table, the muscles of the spine are not called into action; for, while the head is in this position, it rests or is supported by the ligament of the neck. The ligament, being thus kept constantly on the stretch, becomes lengthened, instead of being made more contractile, as muscles would be; and hence the stoop is increased. When this is combined with the consequences of the want of muscular action, the deeper ligaments, which bind the upper vertebræ, gradually yield; if the operation of these causes continues for a certain time, the bones and cartilages themselves become altered in shape, and consequently an almost irremediable stoop is produced *.

This view derives confirmation, from what may be observed in the shape of the tailors in some parts of Germany, who, instead of having the erect figures of London tailors, are quite bent. On inquiring into the cause, we find that, instead of sitting as tailors do in this country, a hole is cut in the table, and a seat is placed within it; so that their position, while working, becomes nearly the same as that of persons who stoop while sitting at a table.

It may, perhaps, be objected, that labourers, and especially the vine-dressers in France, are remarkable for the complete arch which their body forms, although they bend while at work as much as the tailor does. This may also be explained; for in the labourer the bend is produced by the pelvis rolling on the head of the thigh bones, while in a person sitting as a tailor the pelvis continues nearly fixed, and the bend is in the vertebræ on the pelvis.

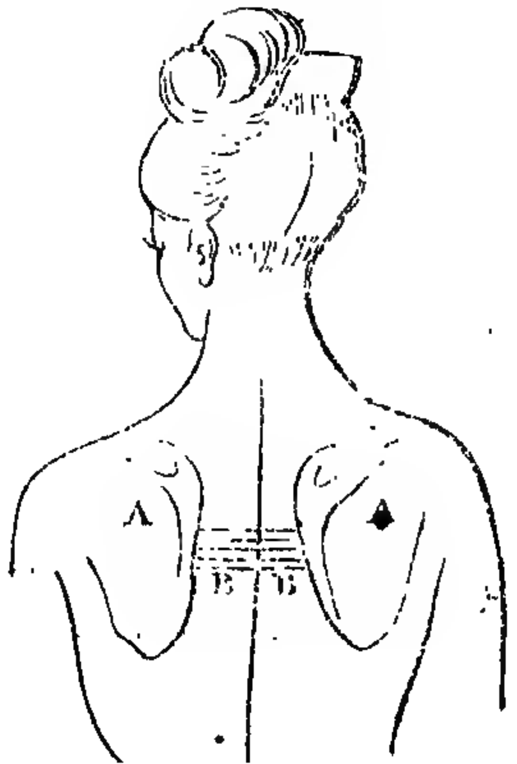
The erect figure of the Turk perhaps comes from the manner of sitting which is common among Eastern nations; but the heavy turban, and the spice box slung from the back of the neck, may account in a great measure for the fine figures of the Turkish Jews who frequent the streets of London.

* Elderly persons may recollect how often the girls who worked at *tambouring* were crooked; the present fashionable amusement of embroidering seems to have, in some instances, the same effect.

We may even take the shoemaker as an example of the effect of a particular manner of sitting, and of frequently using the muscles of the shoulders. He is also a little in caricature, but he carries himself better than the tailor, and the cause is obvious. The tailor's figure is very erect, but the right shoulder is generally a little higher or larger than the left, from the constant exercise given to the right arm, while the left rests upon the knee: this inequality of the shoulders is not observed in the shoemaker, because he not only uses both arms equally, but the muscles by which the scapulæ are supported, become so strong by the habit of jerking back his elbows while he works, that his shoulders always appear more braced back than those of any other class of persons: indeed, so characteristic are the figures of tailors and shoemakers, that they may be easily distinguished in a crowd.

These circumstances are mentioned, as they afford familiar examples of the principles on which we ought to proceed, in endeavouring to correct deformities; but it would be ridiculous to propose the position either of the tailor or of the shoemaker, as the best adapted to correct a stoop or falling forward of the shoulders.

The preceding observations apply also to the contrivances usually employed to keep the shoulders back, and particularly to the question of the propriety of using the common back-collar. The effect which this instrument produces in ordinary cases may be easily comprehended by the following diagram.



The part of the back formed by the ribs is not a flat, but rather a round surface; and as the shoulder-blades rest on this, they would fall forwards were they not prevented by the collar-bones; but as these bones are united to the breast-bone by a moveable joint, and as the weight of the arms operates principally on the anterior angles of the scapulæ, both the collar-bones and the shoulders would fall forwards, were it not for the action of several strong muscles which pass from the spine to the scapulæ. But these muscles may be destroyed by any contrivance which supersedes their use. For example, let A A be the shoulder-blades, and B B the muscles which support them. If the scapulæ be brought close to the spine by the straps of the collar, and kept constantly so, there can be no use for the muscles B B. They must consequently waste and become nearly useless, while those on the fore-part of the chest, being excited to resist the straps, will become increased in power; and hence, when the collar is taken off, not only will the shoulders fall forward as in a delicate person, but the muscles on the fore-part of the chest will predominate over those by which the scapulæ should be held back, and *pull* the shoulders forwards.

The spine and the ribs are occasionally bent so as to have some resemblance to the back of a spoon. In such cases, the shoulders not only appear high and round, but the lower angles of the scapulæ project in an extraordinary manner, because the upper and anterior angle is not only unsupported by the ribs, but is dragged forwards by the clavicles which are carried in the same direction with the sternum. When this is to a considerable extent, it constitutes the *contracted chest* or the *chicken breast.* This, in a slight degree, is common in London, and especially among young lads; it may be discovered by the coat having the appearance of being more worn opposite the lower angle of the scapula than at any other part. Such a condition of the chest can only be completely remedied by appropriate exercises; but a collar is here necessary for a time, to keep the bones in the improved condition into which they are brought by the exercises.

These arguments will probably appear sufficiently well founded to prove that a girl, under ordinary circumstances,

cannot hold her head or shoulders back, unless the muscles by which they are naturally supported are in a proper condition. Various contrivances have been proposed to strengthen these muscles. Dumb bells, if managed in a particular manner, are good; skipping, when the arms are thrown backwards and over the head, is still better; the exercises, called Spanish exercises, performed with two long poles, are also useful, but to each of these there may be objections, as they all operate more or less on the spine or ribs, which, in case of a bad stoop, are generally affected.

The following anecdote will, perhaps, set the question of the propriety of wearing the back collar in a correct point of view. A surgeon was consulted by a gentleman, who is now one of our first tragedians, as to the best mode of correcting a stoop which he had acquired. The surgeon told him that neither stays nor straps would do him any essential good, and that the only method of succeeding was to recollect to keep his shoulders braced back by a voluntary effort. But the tragedian replied, that this he could not do, as his mind was otherwise occupied. The surgeon then told him that he could give him no further assistance. Shortly after this conversation, the actor ordered his tailor to make a coat of the finest kerseymere, so as to fit him very tightly, when his shoulders were thrown back. Whenever his shoulders fell forward he was reminded by a pinch under the arms, that his coat cost him six guineas, and that it was made of very fragile materials; being thus forced, for the sake of his fine coat, to keep his shoulders back, he soon cured himself of the stoop. The surgeon was much obliged to him for the hint, and afterwards, when consulted whether young ladies should wear shoulder straps, permitted them, on condition that they were made of fine muslin, or valuable silk, for tearing which there should be a forfeit.

An inquiry into the manner a girl should sit, may appear trifling to those who have not been in the habit of seeing many cases of distortion of the spine, but it is intimately connected with the present subject, and is really of considerable importance. The question has been disputed; one party insisting that girls should always sit erect, while others are advocates for a lounging position. It is not difficult to show that both are

wrong;—when a delicately formed girl is supposed to be sitting erect, she is generally sitting crooked: to a superficial observer she may appear quite straight; but any one who will sit on a music stool, and endeavour to keep his body in a perpendicular line for ten minutes, will be convinced that it is difficult for even a strong man to sit as long as a delicate girl is expected to do, without allowing the spine to sink to one side or to fall forwards.

The attempt to sit erect beyond a certain time is injurious, for although bending the spine occasionally is useful rather than hurtful, yet when it is done involuntarily, and when the bend is attempted to be concealed by an endeavour to keep the head straight, there is danger of the spine becoming twisted. Indeed, a double curve is generally the consequence; there is first a bend to one side, to give ease to the fatigued muscles; and then, to conceal this, there is a second curve that is necessarily accompanied by a slight twist in the vertical line of the whole column.

The proposal to allow children to sit in a crooked or lounging position seems to have been founded on the idea that all the muscles are more relaxed in this way than even when the child lies at full length on its back. This notion is certainly incorrect, and such a mode of sitting is injurious; for even were the muscles more relaxed by it, the bones and ligaments acquire such a shape as necessarily produces distortion.

It may naturally be asked how a girl should sit, since it would appear, that whether she is in an erect or stooping posture, she is equally in danger of becoming crooked. As sitting, in the manner generally recommended, affords little or no support to one who is weak, the safest answer would be, that a delicate girl should not sit for even more than five or ten minutes without having some support to her back, and when she is fatigued, that she should lie down or recline on a couch. But as it would be very annoying to a girl not to be allowed to sit up except for so short a time, and as a couch is not always at hand, we must endeavour to show how a delicate girl may remain in an upright posture for a reasonable time without incurring any risk of becoming crooked. This leads to an inquiry into the merits of the chairs which are at present generally used by children.

Young ladies are often obliged, while at their music lessons, to sit upon those chairs, which have high backs, long legs, and small seats. These chairs are said to have been invented by a very eminent surgeon, and are intended, either to prevent distortion, by some supposed operation on the spine, or as the most effectual means of supporting the body. It is difficult to imagine how a chair of this description can effect the first purpose; and to discover how far it is calculated for the second, the reader should make the experiment on a chair of the same proportion to his figure, as the chair in question is to that of a little girl. He will find that if the seat or surface on which he rests is small in proportion to his body, the chest will, after a time, either fall forward or to one side, unless he exert himself to a degree that is very fatiguing. Indeed, if the seat be at the same time so high, that the feet do not rest fairly on the ground, but dangle under the chair, a forward position of the head is almost necessary to preserve the balance of the figure*.

The objections to such chairs have been met with the assertion, that girls feel remarkably comfortable in them. This is no argument in favour of their use, for it is not uncommon for a girl who has seven or eight pounds of iron strapped upon her body and next to her skin, to say the machine annoys her so little, that she does not care how long she wears it.

But whether this chair is agreeable or not, it is easy to show that it is not calculated to give much proper support to the body, and that it is almost impossible for a delicate girl to sit long in a natural or easy position upon it.

It may be allowed, that the chair which we consider the most comfortable, that is, the chair which affords the most support to the body, should, if made in proper proportions, be the best for a delicate girl. In such a chair, the *seat* should be scarcely higher than the knees (thus permitting the whole of the foot to rest on the floor), and of such a size, that on sitting back, the upper part of the calves nearly touch it. This form of *seat* is very different from that of the chair alluded to, the *back* of which is also equally objectionable, for, instead of being in

* It must be almost unnecessary to remind the reader that if the knees are bent in standing or walking, there is a curve in the spine at the same time.

some degree shaped to the natural curves of the spine, it is made nearly straight, and projects so as to push the head forwards. A delicate girl should always sit so as to rest against the back of the chair, and, if the lower part of her spine is weak, a small cushion will afford great relief. As it is quite a mistake to suppose that the shoulders, if raised in any other way than by the action of the muscles, or by the curvature of the spine and ribs, will continue high, there is no real objection to a girl who is delicate being supported by an arm-chair; for, by occasionally resting on the elbows, a considerable weight is taken off from that part of the spine which is the most likely to yield.

These observations refer only to the manner in which delicate girls, whose spines are still straight, should sit: when the spine is actually distorted, it will be necessary to use other means.

A Critique on the Aplanatic Object-Glasses, for diverging Rays, of Vincent Chevalier, ainé et fils. *By* C. R. Goring, M.D.

The curiosity of many will doubtless be excited, as to what our neighbours, the French, ever foremost in the pursuit of glory, both in arts and arms, have been doing in the affair of achromatic object-glasses for microscopes. With the highest satisfaction I find myself enabled to state, that Messieurs Chevalier, (ainé et fils,) No. 69, Quai de l'Horloge, Paris, have rivalled our own artists, in this branch of the manufacture of optical instruments.

Mr. J. Lister, actuated by a most laudable zeal for the prosecution and advancement of optical science, as it concerns microscopes, caused me to order for him one of Messrs. Chevalier's instruments, *in Mr. W. Tulley's name*; for, as Mr. L. wished that Messrs. C.'s pretensions should be fairly and thoroughly scrutinized, it was but fair that the latter gentlemen should be stimulated to do their utmost, by a consideration of the science of their customer. A critical examination of the object-glasses of this instrument (for making which every facility was afforded me by Mr. L.), forms the subject of the present paper.

I here, then, enter upon the discussion of the merits and demerits of the objectives of the said instrument *, these being much more perfected than those of another, of previous make, which I saw in the possession of Mr. Howship, of Great George-street, Hanover-square, to whom I received a letter of recommendation from Mr. Spilsbury, of Ball-Haye. To the signal politeness of these gentlemen, in furthering my views, I am greatly indebted.

Four object-glasses accompany Messrs. Chevaliers' instruments (at least those marked *perfectionnés,*) usually rated at the following foci: 14 French lines, 10 ditto, 4 and 4: the two latter combine together at will, and give a focus of two lines.

14.) Focus about 1.42 of an English inch, clear aperture 0.31, original aperture as reduced by a stop, 0.10.

It is perfectly achromatic with its clear aperture, and may be used without a stop on most transparent objects; requires to be cut off to 0.23, to give the necessary distinctness for opaque ones.—(When I speak of the apertures which C.'s lenses will bear, I must be understood, here and elsewhere, only with regard to the *middle of the field of view*, or rather that part of it where the distinctness is greatest †, for double object-glasses give the central rays only correct, and confuse the oblique ones very much, for which reason, conjoined with the small apertures they admit of, they were abandoned by Mr. Tulley, for the triple construction, the true and regular form for the microscope.)—There is an excess of spherical aberration in convex lenses; neither are the glasses well ground, or centered, or duly adjusted. The concave of this object-glass is tarnished, and there are traces of seediness in the cement, which is, indeed, to be seen more or less in the whole of them.

10.) Focus about 0.91, clear aperture 0.23, original stop 0.09.

* The objects employed by me in looking into the defects and excellencies of these glasses, were an artificial star, and a piece of enamelled dial-plate, the phenomena presented by which, *when put out of focus*, incontestably warrant the judgment I have pronounced upon them, as any true optician will admit.

† When an object-glass is out of adjustment, its maximum of distinctness is not in the centre of the field of view, but somewhere else, according to circumstances.

This object-glass is under corrected in point of colour, and wants to be made longer in the focus to be achromatic. The excess of uncorrected spherical aberration is in the convex lens; the glasses are not well ground, centered, or adjusted; the same appearance of tarnish as in 14; bears its clear aperture for the middle of the field on most transparent objects, but must be cut off to 0.2 for opaque ones.

Both of these object-glasses are ineffective upon test objects, from want of sufficient power and aperture.

4.) Focus about 0.43, clear aperture 0.23, original stop 0.09, perfectly achromatic. The uncorrected spherical aberration is in the concave; centering and grinding very fine, but in very bad adjustment; shows some transparent test-objects pretty well with its clear aperture, and, cut off to about 0.16, performs well on many opaque ones.

2.) Focus about 4.7, clear aperture 0.21, no stop, perfectly achromatic, surplus of spherical aberration in the concave as before; centering and grinding very fine; adjustment tolerable; in other respects very similar to 4. This object-glass being adjusted, does more singly on test-objects than any other, and carries an aperture of 0.16 well on opaque bodies, showing the lines on the diamond-beetle's scales strong and well cut out.

Combination of 4 and 2—(quadruple.)

I am happy to be able to speak in terms of almost unqualified approbation of this composition. It, of course, surpasses the performance of any single triple-glass, *on those test-objects which require extravagant angles of aperture.* The field also is good all over, or at least would be, if the glasses were in adjustment, which is the only drawback upon it. The focus of the combination is only 0.26, yet it performs admirably on transparent test-objects with its naked aperture of 0.23, and is very fine on opaque ones with 0.16, and doubtless would carry 0.2, if the adjustment was duly carried into effect.

Messrs. C. have, I think, most assuredly here hit upon one of the very best *compositions* for the object-glass of a microscope; all the imperfections of double object-glasses, *taken singly*, are here done away, while their thinness and agglutination into one mass allows of their combining together almost as if they were simple plano-convex lenses, leaving moreover abundance of space for the illumination of opaque bodies.

I must here state, that Messrs. C.'s object-glasses are all stuck together, I believe, with fused gum-mastic, or, perhaps, with very thick mastic varnish. This practice seems, in theory, to be bad, most especially if the curves united together are not of the same radius; nevertheless, practically speaking, the process of soldering seems to me to do more good, by the obliteration of two surfaces, and by keeping the glasses immovably adjusted, than harm in any other way. I cannot, in fact, discover any very sensible difference in the optical performance of these small achromatics, whether stuck together or not. I *fancy* that they have a little more light and clearness when cemented, (as they certainly should have,) but cannot be very positive. I hold it as a maxim in practical optics, as in our common law, " de rebus non *apparentibus* et non existentibus eadem est ratio."

I may observe, that Mr. Lister has combined that marked 10, with 4, and finds the performance proportional to that of 4 and 2.

It will be remarked that Messrs. C., from an apparent ignorance of the value of aperture, and perhaps impressed with the too common and prevalent idea, that, having once obtained distinctness and achromatism in their object-glasses, every thing else might be accomplished by a condensation of artificial light, have reduced their apertures to such a degree, as to render their instrument as ineffective upon test-objects as a common compound; for when the opening of an aplanatic glass is cut off to the same diameter as a common one, it shows *nothing more*, though it will certainly exhibit objects far *more satisfactorily*. Upon the apertures of microscopic lenses their effects entirely depend, as was remarked a long time ago by the great Huygens. An achromatic glass is more valuable than another, merely on account of the larger aperture it will bear, without causing aberration, and consequent indistinctness. Those who are in possession of Messrs. C.'s microscopes should get the stop behind the object-glasses turned out, and procure others to be used ad libitum, according to what the goodness of the object-glasses will permit.

I feel myself called upon, however, to state, that, since the completion of Mr. L.'s microscope, Messrs. C. *have enlarged the*

apertures of their object-glasses to the requisite angle, and have moreover *arrived at the true method of adjusting them*, so that they are *now* free from those objections which applied to Mr. Lister's, and are in all respects unexceptionably finished.

I know not if any dispute will ever arise hereafter, as to who is to be considered as *the original maker of effective aplanatic object-glasses for microscopes* *. It is of very little consequence in the present instance, for it so happens that Mr. Tulley and Messrs. Chevalier have been so totally unconnected with each other, and have worked upon such totally different principles, that it must be evident, on the most superficial consideration, that both are entitled to the honour; nevertheless I apprehend it can be proved that Mr. Tulley made an effective one before the Chevaliers, having completed his in March 1824. The date affixed by Chevalier to his first instrument is 1825†.

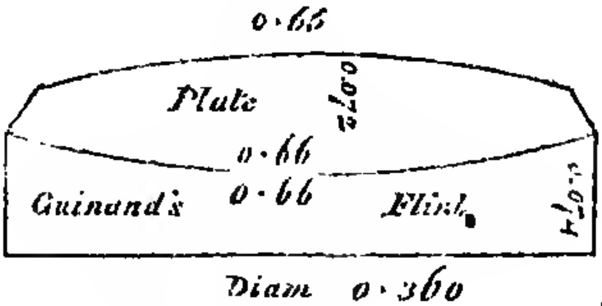
It requires moreover a stretch of complaisance, not to be expected on this side the Channel, to be enabled to admit that the best double object-glass is (*taken singly*) effective; or that, in consequence, Chevalier made an *effective one until he had enlarged his apertures, and combined two together*‡, which combination is not to be met with in his primitive instruments. Mr. Lister, (to whom the public is mainly indebted for the present eclaircissement concerning Chevalier's instrument,) has, by a peculiar method of his own discovery, measured the

* The question must naturally resolve itself into this point, for achromatics for microscopes, (as they are called,) were made by Dollond, Martin, and Pollard, many years ago; the only objection to them was, that they were *not effective*, consequently, *nominal only*, and useless. I defy any man to produce an *effective* object-glass, which can satisfactorily be proved to have been made before 1824.

† See the *first* instruction published by Messrs. C. along with their microscopes: "Microscope Achromatique selon Euler," &c. I apprehend there is a misdate in Messrs. C.'s letter at the end of this paper, where they state they have made the achromatics *since* 1824, *with four objectives*, which implies that they also made them before that period, but in another way.

‡ Mr. C. Tulley recommended the combination of two achromatics together from nearly the beginning of his son W.'s labours upon them, who rejected the idea, together with myself, as giving rise to too great a complication, *always supposing that triple glasses must be used.* I myself combined two triple achromatics together, for experiment sake, in a very early stage of our proceedings, but liked not the result, though effects were certainly produced by the composition, which could not be obtained from the best individual triple one.

curves, thicknesses, and diameter, &c. of that marked 14, which I here give (unfortunately one of the least effective of the set,) however, in all probability, the rest are constructed on the same principle; the annexed drawing by Mr. L. will sufficiently explain itself. Nothing can surpass the beautiful simplicity of



Chevalier's, or rather Euler's, curves, which, it will be observed, are all alike*. The production of *deep* achromatics must ever be a task of some difficulty, even to those who thoroughly understand their humours and punctilios; and unscientific artists will, I think, be much more likely to succeed on the French plan, than the English one. Two double object-glasses, by themselves, are very poor things; but, when combined, perform admirably, and will, I believe, (if the three curves are of equal radii,) be far more easily executed than one of the triple Tulleian construction. The dense flint-glass of Guenard, or Frauenhofer, however, will be an indispensible requisite, from the nature of the curvatures. A triple Tulleian object-glass, and a thin double one of Chevalier, (a composition first conceived and adopted by Mr. Lister,) form an excellent combination, and give a very vivid light, without softness, dulness, or nebulosity. This, I think, is the extreme number of glasses which ought to be tolerated. Let it never be forgotten, that *a really good triple glass will bear an aperture quite sufficient for ninety-nine objects out of a hundred.* I myself denounce the practice of combining glasses together, *in all those cases where they are capable of doing their work alone.* I shall always consider it as a clumsy, bungling, and unworkmanlike method of obtaining a short focus, combined

* The theory on which these object-glasses are constructed is contained in a paper of Euler's, published at St. Petersburgh, in 1774. Messrs. Chevalier have caused it to be inserted entire in the "Bulletin de la Société d'Encouragement de Paris," No. CCLIV., for Aug. 1825.

with a large angle of aperture. If a man aims at perfection, and wishes to distinguish himself in this branch of optics, let it be done by working perfect triple glasses of 0.2 and 0.3 inch focus, with 0.1 and 0.15 of perfect aperture, like those of Mr. W. Tulley and Mr. Dollond, or deeper still, if he is able; and it is with the most cordial satisfaction that I am enabled to inform my readers, that Messrs. Chevalier (duly appreciating the regular triple construction as the true form for the microscope) have applied themselves diligently to the manufacture of this species of objective, from which they have already had excellent results. It is a fundamental principle, that all superfluous refractions and reflections are to be avoided in the construction of optical instruments. As a radical reformer of microscopes, I can tolerate no abuses in them, or show any quarter to their abettors. Messrs. Chevalier have also undertaken the manufacture of achromatic and catadioptric microscopes, after the fashion of those made by Professor Amici, of Modena, which were so much and so justly admired by the cognoscenti of this country.

It only remains for me to observe, that though two double-cemented object-glasses form the most perfect *combination* from the fewness of their surfaces, and consequent brightness of their image, yet a fusion between the Tulleian and Eulerian constructions seems to be the *most convenient* for general use; by this, of course, I mean a triple glass with a double one, to apply before it occasionally, à la Lister. Mr. Dollond and Messrs. C. have demonstrated that two object-glasses *may be combined with the best effect, which are both good, and work well separately*; but Mr. Tulley has constructed a double one, which, useless by itself, when applied over a triple one, (made to act singly,) corrects that excess of spherical aberration in the concave lens, (by its own excess in the convex,) which, when the aperture is large, is the eternal vice of all the best single and compound object-glasses for diverging rays which I ever saw. This is perhaps the ultimatum of improvement, though a quadruple one, on the same plan, might have the advantage in greater light and clearness, from its simplicity, and the paucity of its surfaces.

The quadruple or quintuple object-glasses are those which

are best adapted for the solar microscope, for they give a full-sized field of view to this instrument, good to the edges, which no single object-glass will do, as I have had occasion to remark in my paper on Mr. Tulley's aplanatics, unless converted into a compound, by means of eye-glasses, &c. This popular and highly amusing instrument will now receive the utmost reformation and improvement of which it is capable, and become truly scientific in its construction: hitherto it has been a mere toy, but one degree removed from a magic lantern.

I shall now allow Messrs. C. to say what they can for themselves, and to detail the various modifications which they have introduced into their instruments, since they executed Mr. Lister's order, by giving a translation of a letter I have received from them on the subject, and shall conclude by expressing a hope that no national or illiberal feeling has entered into the composition of this critique, and that I have used my oil, vinegar, and pepper in correct proportions.

"*Paris, Oct.* 15.

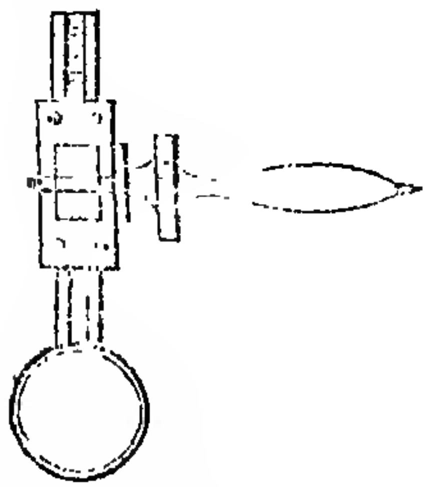
"Sir,

"Accept our thanks for your extreme complaisance in offering to publish the results obtained by us in the construction of microscopes. Since the order executed for Mr. Lister, we have improved those instruments last completed, by greatly enlarging the diameter of the illuminating mirror, in order to obtain still greater light. The prism for opaque bodies is diminished about one half, and by a small modification in its mounting is rendered more serviceable: the diminution of the length of the body has enabled us to augment the magnifying powers by different eye-glasses, and the four double object-glasses placed in a better mounting can be used separately or in superposition, according to the pleasure of the observer, either to form quadruple objectives, or even to combine in a mass together. This last arrangement produces a great accession of magnifying power, without injuring the clearness of the image or arresting much light.

"You see, Sir, that we have the pleasure of coinciding perfectly in your opinions about improvements, for we adopt the quadruple object-glass as the best, and we give three changes

of eye-pieces. The double motion given to the body of a microscope is, in our idea, the defect of all those hitherto constructed; for, as the optical part should remain perfectly centred with the mirror and the diaphragms, it is evident that the least derangement of it from this position must destroy the perfection of the image: the stage then only should move* without affecting the diaphragms or the mirror, and in this we

* This is the theoretical view of the case, the practical one is different, as frequently happens. *Opaque objects* are not affected at all by the eccentricity of the axis of the body: nor can I recollect that I ever felt any particular inconvenience from the motion of the optical part, even with transparent subjects, unless it was thrown *very much indeed* out of the axis of the illuminating mirror. On the other hand, it is notorious that *living* aquatic insects and animalcules are the most *popular* and *entertaining* objects which microscopes can show. These are, for the most part, abundantly restless; and if the stage on which they are placed has any motion, their natural unquietness is so much exasperated, that it becomes almost impossible to get a good observation of them at all. I once set to work at making some drawings of a variety of new and original objects of this class (which, I trust, will one day be published) with a microscope having all the requisite motions applied to its stage, and am confident that I had thrice the labour fairly appropriate to the execution of my task from this oversight. The very tremor produced by the transition of a carriage in the street, is frequently sufficient to unsettle live objects when disposed to be still and quiet, and put them in a fidget for a quarter of an hour. It is very unfortunate that the mountings of optical instruments are made in general by mere mechanics, who seldom or never observe with them, and consequently know not the exigencies which occur in practice. It is still more unfortunate, that in the science of fitting up microscopes, an ounce of a man's own wit is worth about a ton of his neighbour's. Was it not that I dislike to verify this adage myself, I should recommend the following motion to be applied *to the body*:—let the socket of the arm which carries it have a smooth rotatory motion on the head of the bar in the usual way, conjoined with another horizontal one produced by rackwork attached to the said socket. Let the pinion which belongs to the latter movement be made very strong, so that a lever about three inches long may project from its centre: this is to be held in the hand, the thumb and index finger operating on the rackwork, while the two little fingers give a rotatory motion by working the lever end. *This rapid double motion is here completely under the command of* one hand, *while the other is at liberty to adjust the focus.* I know of nothing better for general purposes, or in particular for following the motions of live insects, &c.; but when *only inanimate ones* are to be the subject of microscopical study, I prefer the motion of the stage, for the reasons stated by Mons. C.



have well succeeded in the construction of the microscope of Sig. Amici. But all these arrangements much augment the price, and an observer ever so little practised will always find the object easily enough by means of his hands.

"The prices of our achromatic microscopes are as follows:—

"*Achromatic microscope*, like Mr. Lister's—300 francs.

"The same, with the latest improvements, three eye-pieces, and camera lucida for drawing the magnified objects—400 francs.

"*Amician Microscope*, one horizontal achromatic, the stage giving all the motions to the object, with a micrometer screw, five eye-pieces, two camera lucidas, hand magnifier, frog trough, accessary apparatus, &c. *One catadioptric microscope*, mounted on the same stand, and adapting itself to the same apparatus; the two instruments inclosed in a mahogany case—1000 francs.

"We trust that the very moderate prices of these instruments, together with the care which we bestow on their construction, will procure us orders for them. Their superiority has been duly recognized by the jury of the Exhibition of the Products of Industry, which has been pleased to decree to us a silver medal.

"The Amician achromatic microscope is composed of a tube seven inches long, at the extremity of which is placed a prism, which reflects at a right angle the rays which come from the object-glass, composed (*as in our microscopes executed since* 1824) of four double object-glasses, which may be used separately, or two, three, or four at a time. The stand is a square bar, which has a rackwork, carrying a moveable stage, which, by means of adjusting screws ingeniously disposed, permits an object to traverse the field of view in every direction. This disposition gives the power of determining the real dimensions of objects submitted to observation by means of the micrometer screw, which is placed at the side, while the camera lucida affords the means of drawing their outline, and consequently of measuring the magnifying power.

The rays proceeding from the object which have passed the object-glass, and have been rendered horizontal by the prism, are received by different eye-pieces disposed after the manner of

Ramsden. Their power can be varied. Each instrument carries six, five of which can be attached at pleasure either to the catadioptric or the achromatic. The deepest belonging to the reflector is a single lens of half a French line focus, and the most powerful of the achromatic is a line and a half.

"Such, we think, are the details which you required: we wish that they may prove agreeable to you.

"We beg you to accept the assurance of the high consideration with which we are, Sir,

"Your very devoted Servants,

"VINCENT CHEVALIER, ainé et fils."

"69, *Quai de l'Horloge*."

On the Existence of Chlorine in the Native Black Oxide of Manganese. By John M'Mullen, Esq.

IN the paper relating to this subject, which the editor of the Quarterly Journal of Science obligingly inserted in the forty-fourth number of that work, I described some experiments which I had made, to show that chlorine is uniformly evolved from the Native Black Oxide of Manganese by the action of sulphuric acid, under certain circumstances, which I endeavoured to detail with strict accuracy, so as to prevent any mistake or failure in the event of the experiment being repeated.

Upon this paper Mr. Richard Phillips has made some observations in the Philosophical Magazine and Annals of Philosophy for April last, to which I am desirous of briefly adverting. He says, "Mr. M'Mullen having observed, when sulphuric acid is added to peroxide of manganese, that chlorine is evolved, he conceived it might be derived from an admixture of muriate of manganese, iron, or copper; but having washed some of the peroxide of manganese with water, he did not find that any chloride of silver was precipitable from it; he, therefore, concluded that the peroxide in question contained no muriatic salt." If Mr. Phillips will take the trouble to refer to my paper, he will find that this is by no means the statement which it contains. I observed that, in order to separate any soluble

muriates which the oxide used might, in the first instance, have contained. "I washed it every day, for three weeks successively, using sometimes hot and sometimes cold water: *at the end of that time*, I tested the water, which was *then* decanted from the washed manganese, by the nitrate of silver, but without finding the slightest appearance of precipitated chloride. I then poured upon the manganese four times its weight of dilute sulphuric acid; allowed the mixture to stand *for about four weeks*, occasionally agitating it, and at the end of that time, found, when the dilute acid, now of a deep crimson colour, *was removed from the subsident manganese*, and the latter agitated, that the most decisive evidence of the presence of chlorine was exhibited in the vapour evolved from it." I stated further, that I had "carefully preserved this particular mixture, and that after a lapse of more than twelve months, the residuary manganese, when the supernatant acid was removed, continued to evolve chlorine."

In this experiment my object was effectually to purify the manganese used from any soluble muriate which might by possibility have been mixed with it: I did not, however, test the water *first* used in washing it, but merely that which was *last* removed from it.

Mr. Phillips proceeds to observe that he had prepared some observations, and at considerable length, *to prove* that the author of the above paper has been completely misled by "forced analogies" and "erroneous experiments;" but that it *afterwards* occurred to him, that it would be better to show, in a few words, the real source of the chlorine in question, the evolution of which from peroxide of manganese *he had noticed some time previous to the publication of my papers*. That with this view he had procured various specimens of the peroxide of manganese, (one of them in the crystallized state,) which were reduced to powder, and on the addition of sulphuric acid, chlorine was evolved from each. That he then washed separate portions of them with distilled water, and on the addition of nitrate of silver to the washings, chloride of silver was immediately precipitated: sulphuric acid being poured upon the washed peroxide, no chlorine whatever was evolved. That he further added sulphuric acid to an unwashed portion, and to one which had

been washed, and referred both to a *bystander*, who immediately detected the odour of chlorine in the former, but not in the latter. He then proceeds to show that the specimens of manganese which he had made the subject of this experiment contained a portion of lime, and he infers that the black oxide of manganese consequently contains muriate of lime.

Mr. Phillips asserts that I have been misled by erroneous experiments. My reply is, that the experiment to which he refers, and which I have recapitulated, was carefully made, and is truly and faithfully detailed In what, then, is it erroneous? It is not incompatible with that which he has produced as a refutation of it, inasmuch as he did not wait the result for which the perusal of my statement should have prepared him, and which he clearly should not have anticipated. He states that chlorine was not evolved from washed manganese at the instant when sulphuric acid was affused upon it. This is not a contradiction of my statement: I affirmed that, after washed manganese had been exposed to the action of sulphuric acid for a very considerable period, I distinctly observed the evolution of chlorine, and that for twelve months afterwards, *under the circumstances detailed*, this continued to be the case:—all this I deliberately re-assert. I have frequently met with specimens of manganese which, upon the first affusion of sulphuric acid, gave off chlorine; but *in general*, as far as my experience goes, *this is not the case:* were it of uniform occurrence, and that the mixture of sulphuric acid and oxide of manganese rendered chlorine evident to the smell, the fact *could not have remained unnoticed till now.*

The observations of Mr. Phillips, to which I now refer, did not come into my hands till about three weeks ago. It fortunately happened that I had still preserved the specimen of washed manganese upon which sulphuric acid had been affused, under the circumstances already recapitulated, and it occurred to me that this would afford occasion, as decisive as I could desire, to put the accuracy of my original experiment, and the conclusions drawn from it, to further proof. I am bound to premise that this specimen of manganese, after having been in the first instance washed and subjected to the action of sulphuric acid, as already mentioned, has ever since remained in

the vessel in which the experiment was at first made, covered with dilute sulphuric acid,—a period of more than eighteen months; and it will scarcely be doubted that, in the course of that long interval, any muriate of lime which it might have originally contained must have been thoroughly decomposed. Upon removing the supernatant acid, the residuary manganese gave sensible evidence of the presence of chlorine; paper stained with the solution of indigo in sulphuric acid was readily bleached by it, &c. I now proceeded to wash the manganese in pure water, and continued to do so until the acid was no longer perceptible to the taste. I then washed it three times successively with distilled water, and after decanting off the fluid of the last washing as closely as possible, added pure sulphuric acid in considerable quantity, and stirred the mixture thoroughly, after it had cooled, with a glass rod. At this time no vapour of chlorine was evident either to the smell or to the usual tests. The mixture was then set aside, and allowed to stand undisturbed for ten days: at the end of that time, when the acid was poured off, and the subsident manganese agitated, the vapour of chlorine was as distinctly manifest as when it was first subjected to experiment more than a year and a half ago.

Dublin, June 11, 1827.

Modern Improvements of Horticulture.

THAT gardening has always been one of the most natural, as well as the most useful occupations of mankind, is obvious: that it has advanced—been retarded—or flourished, according as general taste or wants, or peculiar political, moral, or local circumstances, were favourable or adverse, is also sufficiently evident from all historical testimony;—but in no age has it advanced with such rapid strides towards perfection as it has done within the last fifty years. To bring the modern improvements in array before the reader,—to estimate their advantages in a public and private point of view,—to look forward from our present elevated station to the probable results of continued and extended application,—may be an amusing, if not an

useful inquiry: it may tend to remove barriers which are only imaginary, and by freeing the thinking powers of practitioners from the trammels of custom, lead them forward into that expanse of operative freedom, where much remains for the exercise of the inquiring mind, and experimental hand, in exploring the yet untrodden field of practicability, and calling forth the still latent powers and susceptibilities of pregnant nature.

When we turn to the history of the first ages, we hear of a garden as soon as we hear of man; and though, from the paucity of description, we can only form ideas of such a place from the effusions of the poet, rather than from the detail of the historian, yet, in judging from what still appears of aboriginal scenery, we may conclude with Milton that a garden was a place,

> "A happy rural seat of various views;
> Groves whose rich trees wept odorous gums and balm;
> Others whose fruit, burnished with golden rind,
> Hung amiable, and of delicious taste:
> Betwixt them lawns—or the flow'ry lap
> Of some irriguous valley spread her store."—*Par. Lost.*

If such a place, it required care to rear the tender—to check the luxuriant—correct the irregular—to support the burdened—extirpate the noisome weed—and repulse the browsing animal. Such was the only occupation of the first gardeners: for in those highly-favoured spots, those natural paradises, (some of which still remain in India,) where the groves which formed the habitations also supplied the simple food of the inhabitants; where the cocoa-nut *, with its various liquors, abounded;

* The cocoa palm is rather a gigantic herb than a tree: the stem rises to a great height, of a strongly tough fibrous substance, but never so indurated as timber, though it is used in the construction of houses. It has no branches; but is crowned with from five to seven ample compound leaves, forming an umbrella-like head. The spatha issues from the centre, and soon falls pendent between and below the footstalks of the leaves, where it flowers and ripens the fruit. The nut is enveloped in a thick brown fibrous husk, which opens to shed it when fully ripe. The nut, when opened, yields two liquids, which are nutritious, and accounted delicacies: the first is the milk which runs out; the next is the cream which is procured by being scooped off the kernel with a spoon: this is of thick consistence, and much resembles the cream of milk. After these remains the perfectly-formed layer of kernel attached to the shell, and

where the date, the mango, tamarind, and lime dropped in profusion into the hand; where the melon tribe upon, and the nutritious yam beneath the surface of the bountiful soil, were all spontaneously supplied without care, and without toil:—in such circumstances, neither sagacity to contrive, nor ability to perform, were necessary, further than collecting and preserving those spontaneous gifts of nature.

But population increased; and when mankind became translocated to regions less favourable to vegetation, and where the spontaneous productions of the earth were insufficient for their subsistence, then the business of the planter and cultivator became a necessary occupation; and hence gardening would begin to assume a systematic form.

As improvements, and the times in which they took place, have descended together in continuous and collateral streams, the narration may be divided into three periods, viz.:—From the earliest ages to the beginning of the sixteenth century;—from the beginning of the sixteenth century to the end of the seventeenth;—and from that period to the present time.

We have already hinted at what were probably natural, or aboriginal gardens: the account is so far feasible from the fact, that such places and productions may be met with on the peninsula of India, at the present day: true it is, they cultivate rice, and some few inferior plants, where they have opportunity, and use them along with their wild fruits; but when they cannot procure these cultivated necessaries, (which sometimes happens,) they must rely on the natural productions. It is necessary to add, that some of the castes, from religious principle, abhor the use of almost any kind of animal food; and, therefore, vegetables are their sole support.

From Egyptian and Jewish history, we learn that gardens

which is used along with the liquids as an article of food. But another most pleasant beverage, called toddy, is obtained from this palm, and which constitutes the chief value of the plant. The fruit is sacrificed to procure this: soon as the frond becomes pendent, the extremity is cut off, and a narrow-necked vessel is slung thereto to receive the streaming sap. This, both before and after being fermented, is an agreeable and refreshing drink. It also yields an ardent spirit by distillation, but of which the natives deny themselves the use.

were an appendage of the palaces of their princes, and other great men, for personal solace and gratification; but how far the art was systematized, either in knowledge or practice, history is silent. Throughout the Assyrian, Babylonian, Persian, and Macedonian empires, we learn but little more than that ornamental gardening was carried to an extravagant height in their artificial formation; insomuch that one of the Babylonian princes built what were called "hanging gardens," that is, a vast and lofty pyramidal structure on arches, arcades with terraces surmounted by other arcades, and carried up in gradations to a great height. The terraces being planted with the choicest trees, presented to the distant spectator a verdant hill of foliage in the midst of a large city, and lifted the sovereign proprietor far above the noise and intrusive notice of his vassals below; at the same time, yielding him all the sweets, seclusion, and quiet of the country, even in the purlieu of his palace! The idea of such an ornamented and elevated structure for a mighty sovereign was certainly sublime, and far surpassing all that has been yet done (though it has been suggested by Mr. Loudon) in the western world; and though only a monument of wealth and personal pride, prompted by conjugal regard, and entirely artificial, was certainly proper for the place where it stood, worthy of the prince who erected, and the extensive empire to which it belonged.

Throughout a long-following period, and up to the time of the Romans, we learn nothing particular respecting gardens, only, that among the Jews, they had gardens for herbs, vineyards, and even gardens for cucumbers: but as frequent allusions are made to them, it is probable that gardening had then become a distinct calling, as we find it was among the Romans, as soon as their extensive conquests were secured.

As the arts and arms of the Romans went together, no doubt a very wide circulation of all that was known of gardening in Italy, was transferred thence. Their writers on rural affairs preferred *agri* to *horti*culture; but their sound knowledge of the former shews no inconsiderable share of acquaintance with the principles of the latter; and as their practice, as well as the seeds of their products, would be introduced wherever the climate permitted, it is more than probable they

laid the foundation of British gardening. The rules and examples left by them, were probably continued, with occasional accessions to the stock both of practice and production, throughout the Heptarchy, the domination of the Saxons, Danes, and Normans: but these troublous times were not favourable to the prosecution of the arts of peace; and it is not likely much advancement in the art took place until the Norman power was fully established; and even then their castellated mansions precluded any extensive adaptation of garden, from the necessity of forming the glacis, for the greater security of the baronial hall: and though it is probable that, at this time, not a dwelling, from the regal palace to the cottage, but had a garden of some size or other, yet the best practice was confined to the monasteries, and other religious corporations of those days, all over Christendom. Their education and leisure, their foreign intercourse, their interest in the tithes, and their love of superior vegetables and condiments, on the many days they were restricted from indulging in the consumption of animal food, all contributed to incline the monks to prosecute gardening on the most approved plans. Thus, Italy, Spain, and Germany became famous for their superior culinary vegetables, as France was for improved fruits; indeed, when war depopulated or devastated a country, and when the gardens of the château became a sacrifice to offensive or defensive operations, and when the potageries of the hamlets were trodden under foot and destroyed by a licentious soldiery, the gardens of the religious houses were often spared, and, consequently, many roots and fruits found there an asylum, which was denied them in less privileged places.

In looking over the lists of plants cultivated in those days, we find the names of a great majority of the common sorts now in use, as well culinary, as for the table or the press, with a great addition of physical plants, it being then a prevalent supposition that remedies for all the ailments of the human frame were existent in the vegetable kingdom, if they could be detected; the cultivation and gathering of simples, therefore, was a business which employed many heads and many hands: even the Corinthian pillars of the noble profession of physic were not entirely free from that malaria, which was generated in the fumes of the herbalist's shop!

Ornamental gardening had hardly showed its graceful head; the little that had been done of this in England, was only in imitation of the Italian school, though without their accompaniments of splendid architecture, classical sculpture, and costly fountains. Such a style, in the near neighbourhood of a palace or mansion, is imposing and suitable, but the outskirts of such gardens, which should have been gradually blended with and into the free and beautiful forms of nature, were bounded and deformed by tortuous labyrinths, by complicated folds of nicely-clipped hedges, involving each other for no purpose than affording seclusion from the "licentious eye," or a "maze to the intruding foot."

It may be observed as somewhat unaccountable, the excellent taste of their landscape painters was never transferred from the canvass to their style of ornamental gardening. But so it was: the people who had all kinds of assistance from artists of the first order, and from classical and picturesque association within their own territory, long remained blind to what was so natural and so manifestly within their reach!

In useful and profitable gardening, the fine climate of Italy gave great facility for successful cultivation of a profusion of the finest fruits, and being an advanced post for the reception of all valuable plants from both Asia and Africa, it much sooner than other European countries possessed culinary vegetables in great variety, and of salad-herbs a numerous list.

From the commencement of the sixteenth century the improvements in gardening began to take the form of a system. The increasing splendour of the English court, during the reigns of the virgin queen and her father, and the princely establishments of some of her courtiers, called the art of gardening into notice and repute, and gave an impulse to the yet dormant powers of horticultural practicability. Continental artists were generally employed in laying out the greater works. The sum of their professional ability was chiefly geometrical, an exact knowledge of straight lines, squares, and curves; they could line out a polygonal basin to a hair's breadth, and construct a many-tiered jet d'eau in the midst. Such, however, were the principal features admitted into, and which constituted the style of those days, and continued through that and the succeeding

century. In the latter (the seventeenth), and during the domestic broils which then convulsed the kingdoms, gardening appears to have been, as to style, almost stationary. In the mean time, however, the Reformation had been silently working salutary effects, not only in the deliverance of men from a servile religious thraldom, but also from the dogmas of precedential custom, and by imbuing them with a spirit of independence with respect to others, gave, what was better, a *self-dependence* in exertion, whether of mind or action; and, after a few years of revolutionary excess, and abuse of this inestimable acquisition of mental freedom, at last, in 1688, settled down into that rational state of composure, which, with few interruptions, has happily remained to the present day.

Then it was that gardening, in all its branches, was patronised and encouraged. Tournefort in France, and Ray in England, enlightened the public by their description, enumeration, and classification of plants. Evelyn called attention to the usefulness and national value of forest trees; several authors developed the mysteries of kitchen-garden and orchard management; collections of exotic plants were made, and glass-cases built for their reception; floriculture received a share of the gardener's attention; and in short, there seemed to be, about this time, a general movement by united exertion to gain what had been previously neglected, and complete what had been only feebly attempted.

The accession of William and Mary to the British throne very naturally introduced Dutch gardening and architecture. The old Italian and French styles received very little, if any, amendment. The avenue, the canal, the rectangular clumps and borders, the shelves and slopes, the terrace, with its stairs, were all maintained and extended, the whole surrounded by exactly-clipped hedges, and bedotted with fanciful and unnaturally cut trees. This expensive and ridiculous fashion had its admirers for a time, but at last fell into disrepute, not by a bull or anathema, but chiefly by the keen sarcasm of a Pope!

Kitchen-gardens were improved by additional brick-walls for the more delicate kinds of fruit, as vines, figs, peaches, nectarins, apricots, &c. Hot-beds were in general use, and many hot-houses were erected for different kinds of the above

as well as for the pine-apple. In those days our fruit-lists contained twenty sorts, of which there were many varieties. Of culinary vegetables there were, of roots eighteen, of shoots four, of leaves fourteen, of flowers three, of seeds three, of pods two, and of herbs for all purposes twenty-five.

From that period, the commencement of the eighteenth century, every succeeding year brought forth new objects of the gardener's care, and improved operations for his imitation. The acquirements of natural science, radiating from such a character as Sir Hans Sloane, whose theories were imbibed and confirmed by the practical abilities of Philip Miller, were, at that time, like the orb of day bursting from behind a cloud! Scientific light and practical life were shed on all around, and the foundation was then laid, by their united means, on which has been raised almost all the varied structure of modern horticultural improvement. It would be impossible, as needless, to give the names of the authors, who, from this period, showed themselves in print on the subject of gardening, for, were the respective merits of their literary labours noticed, and the successive discoveries and advances chronicled, the amount would be voluminous indeed. But the following celebrated names cannot, in justice to their memories, be omitted. The great Linnæus was deservedly at the head of the botanical branch of gardening; Miller, with his satellites, Gordon, Lee, and Aiton were at that of practical botany, as well as of all the other parts of operative gardening; and, as a leading orchardist, Kennedy, and many others on miscellaneous subjects, produced respectable directories and kalendars.

The improvement of ornamental gardening kept pace with that of the more useful. Soon as the old style of rigid formality had been exposed, it was exploded; more refined principles of taste prevailed; its outlines became better defined; it was found that there are certain fixed principles in nature, on which the elements of true taste are naturally (not capriciously) founded; that delight and gratification to the eye, or mind, can only arise from the harmony and fitness of the combinations of art or design; that the sensations of beauty and sublimity can only be conveyed by congruous associations of parts to the whole; and that the incidents found in conjunc-

tion in nature should be the objects of imitation of the gardener's, as they had long been that of the painter's art, with this exception, that in the immediate vicinity of the mansion as much of the old style should be retained as will harmonize with the necessary artificial façade of the architecture; but soon as departed from these creations of art, let then appear the varied flow of nature's devious garb.

The art of painting had, in the best schools, proceeded on such principles, and the formation of real scenery was improved from what was so prominent in the fictitious. Some painters, even Claude Lorraine himself, have occasionally erred, from what may be called *exuberance of design*, in producing extreme effect, by introducing lights which never can be seen by day or night, at dawn or twilight; by trees which never existed, and by forms* which had only an imaginary existence. Landscape gardeners, too, in the transition from the tame to the more natural style, have run into error, by imitating admirable incidents frequently seen in nature's works, forgetting that their value springs entirely from their having happened by chance, but, as works of art, lose all their interest, and become insignificant.

We are now arrived on the confines of our own times, of which we will take a general view, and which will sufficiently show the accumulated assemblage of horticultural objects, productions, and knowledge; and which will also give, what was proposed, a comparative surveyof the extent of our improvements.

And first, as to the highest department, Botany. Before the sexual system of Linnæus was fairly established, (though it spread far and wide by the literary labours of Hudson, Lee, Curtis, and several other able contemporary writers, both in England and on the continent,) defects were found in it, and not only as to the terms of distinction, but also to its bringing together, in the classification, plants which appeared, from their

* In Martin's painting of the Paphian Bower, though a fine composition, the roots of the tree, on the left of the foreground, are too much out of the ground. The accidental exposure of roots on the bank of a stream, or high-road, and their buttress-like departure from the trunk, are legitimate objects for the pencil; but their ramifications pourtrayed on the surface of the ground, is as ridiculous as unnatural.

exterior habit and qualities, to have no natural affinity to each other. This Linnæus was aware of himself, and left some fragments of a natural arrangement, but which he did not live to complete. This, or the idea of it, however, was taken up by Jussieu, a French botanist, and completed as far, perhaps, as it can be; and though, in our present botanical publications, both systems are continued, yet it is likely that Jussieu's simplified system will, in time, supersede the other, though the curious fact on which that of Linnæus was founded will never be forgotten, because of its practical use in the amelioration of fruits. Botanical publications, under the various names of Hortuses, Floras, Monographs, and of every country and district under the skies, and since the promulgation of Jussieu's system, monographs, under the titles of Geraniaceæ, Cistineæ, &c., flow in periodical floods from the press, crowd the bookseller's shelves, and thence find their way to every elegant drawing-room in the kingdom.

This additional call on the business of the press, as well as upon the talent of the artist, arises from the fashionable and refined bias of the public taste for this rational and delightful study. To extend botanical collections, and the desire to possess every vegetable beauty, pervaded the whole community: hence expeditions to distant lands by collectors; hence the extension and encouragement of nursery business; hence have sprung up chartered societies and associations for the encouragement of botany and gardening all over the realm; so that vegetable beauties and curiosities are now to be seen in British collections, from every region of the known world.

Neither has the occult subject of botanical physiology been neglected; many curious facts connected with the organisation, structure, functions, and qualities of plants, have been ascertained: but still there remains much for the employment of the naturalist's mind on this difficult subject.

Landscape gardening is not so much "the rage" as it was twenty or thirty years ago: national circumstances, perhaps, may be the cause; but its principles are much better understood. The errors of Kent and Brown, and their followers, have been corrected by the works and writings of Repton, and the critiques of Knight and Price, whose theories have been

carried into practice by London and others; and nothing will prevent the universal adoption of their principles, but the difficulty of giving the foreground from *home walks* the extreme degree of ruggedness so much admired, and even indispensable to the painter. Fern, (unless we can introduce some uncommon foreign variety,) burdock, kexes, cannot be admitted into dressed ground; nor have we any plants in cultivation which would well answer the purpose; true, we have the rhubarb, one or two sorts of thistles, eryngiums, palma Christi, and gourds with their ample leaves: but these would only appear intruders, and misplaced: but much may be done by a judicious disposition of our common shrubs, so as to conceal the traces of the spade and line, and give all our combinations of land, wood, and water that flowing character, which is so true to nature, and so pleasing to the refined eye of taste.

Floriculture, which has been imported from France and Holland, is also intensely followed about London, as well as in our manufacturing provinces. Authors have creditably appeared in this line too; and our annual blows of flowers, both home-cultivated and imported, are at once rich and costly. Tulips, hyacinths, narcissuses, ranunculuses, and anemones, are the principal bed flowers: but roses, stocks, dahlias, chrysanthemums, and even poppies, are out of number. Flowering shrubs, both within and without doors, are eminently rich and various, and astonish as much by the splendour of their colours as by their elegant forms and number.

Orcharding has declined during the last fifty years: first, because of the gradual deterioration of the trees, and precariousness of the crops; next, from the improved way of agricultural labourers' manner of living. This change renders the use of small cider and perry less necessary in a farm-house, causing an increased consumption of malt liquor; and this again, occasioning a greater demand for barley, at once pleases both the farmer and the Chancellor of the Exchequer. Thus the cultivation of orchard fruit (except cherries *) has greatly fallen off; and the decayed state of the old, and difficulty of

* It is said that many of the Caroon cherries brought to Covent-garden market, are bought up for the purpose of colouring wine on the Continent.

raising new orchards, has given a check to such exertion, except in places at a distance from the metropolis, where orchards have suffered less from decay, and where the habits of the cider drinkers are more inveterate. Mr. Knight, the President of the Horticultural Society, has written copiously on this subject, and very properly considers the cultivation of orchard fruit as a national object; and by his example has done as much, nay, much more, than any other gentleman in the kingdom, to restore our orchards to what they used to be, and what they may be,—and it is hoped his excellent instructions will not be thrown away.

Kitchen-gardening, the most important and useful branch of the subject, next demands attention; and here we are gratified with a fine display of the efficacy of perseverance, the success of experience, and the triumphs of skill. In every month of the year, in spite of the winter's frost or summer's sun, our tables are supplied with wholesome and agreeable vegetables:—of roots we have fourteen sorts; of stems, shoots, and leaf-stalks, seven; of leaves, eight; of flowers, four: of culinary fruits we have sixteen; of seeds and pods, six; of condimental herbs we have twenty-nine; and of herbs and seeds for confections there are seven, besides various fruit: of roots, leaves, flowers, and fruit, for salads, there are in cultivation twenty-two kinds; and various sorts of plants for medicine and distillation.

Of table-fruit there are above twenty different species, and of these numberless varieties, extending to several hundreds, or even thousands, of various excellency and value.

A few tropical and foreign fruits, not included in the above, have been cultivated in tolerable perfection in Europe within these few years, viz. the Chinese loquat and litchee, the custard apple, mangosteen, and mango, &c.; and there is no doubt, if these fruits could be worked on some hardier kindred stock, and a suitable place formed for them in a stove, they might be cultivated with the same success as the anana.

In the forcing department of gardening, wonders have been accomplished. By this application of art, we appropriate to ourselves an almost perfect imitation of any of the warmer cli-

mates: heat, that powerful agent in the development of vegetation, we can have in any degree, by stoves, by fermenting substances, and from the steam of boiling water: light, a no less necessary agent in the maturation of fruits, we combine with the former, by glazed houses and frames.

The various expedients for obtaining the necessary degree of heat, are, first, the most simple method of a stove, with its flue passing through or round the floor of the house, and this for warming the air within; but in this case, as the roots of the plants do not sufficiently, it is supposed, receive the proper degree of heat, various fermenting substances, as recent stable-yard dung, tanners' bark, oak and other leaves of trees, &c. are formed into beds, on which the compost of earth is placed, as in hot-bed frames, or in which the pots containing the plants are placed, or plunged, as in a hot-house. To obtain the same effect, borders within houses are formed for the roots, having an excavated heat-chamber beneath, supplied by simple stove flues, or from the fermenting substances above named, or from steam admitted for the purpose. This mode of supplying an equal degree of heat to the roots, as well as to the leaves and branches of a plant, is plausible, and cannot be far wrong, because it has been attended with success: but there is, perhaps, more attributed to it than it deserves, because the region or stratum of the soil, which is naturally occupied by roots, differs, in respect of temperature, much less over the whole surface of our globe, than is commonly imagined. The heat of the air in different latitudes ranges from several degrees below zero to 110 degrees of Fahrenheit's scale; but the temperature of the earth eighteen inches below the surface, it is probable, does not vary more than ten or fifteen. In England spring water varies only two degrees, viz. from 42 degrees in summer to 40 degrees in winter; and the effects of our hardest frosts very rarely penetrate deeper than nine inches; but it is necessary to observe, that, in such cases, as well as in hotbeds, we *force* as well as defend; and probably, by such mode of applying heat and moisture, nutritious gases may be communicated, which may be no small advantage. Besides, the atmosphere of the house can (as is done) be impregnated with the same qualities and degrees of heat and humidity (a.

most necessary accompaniment*,) which may be generated below.

Light is a most potent agent in the maturation of vegetables: united with a moderate degree of cold, it is much more effectual in progressive vegetation, than the necessary degree of heat with darkness. Exposure to light is indispensable to plants: and, therefore, our glass cases are formed to admit as much as possible. Within these few years, the endeavour to gain an accession of light by reducing the dimensions of the wooden scantling of hot houses, suggested the idea of metallic frames; and for the concentration of the sun's rays, horizontal as well as vertical curvilinear roofs have been constructed. Lightness to the eye, durability, imperceptible expansion, and glazed with panes, cut like segments of circles, to facilitate the passing off of condensed water, with complete command of ventilation, are an assemblage of properties, always as desirable as necessary; and as they may be cast in the most elegant forms, and protected by paint, they add greatly to the ornament of the garden. Beautiful as these buildings are, some little disappointment has taken place respecting them: it has been experienced, that the intensity of the sun's light, or heat, has been found detrimental to the tender inmates, and that shading is as necessary in bright, as light is in cloudy weather. Certain it is, that in the winter season, when light and heat are most desirable, no fear need be entertained from this circumstance; and it ought to be considered, that in our summer, we have at least, daily, *four hours* more sun than intertropical plants have at home: of course, they have less time for their evening's repose, (which all plants more or less require); besides, it should be thought of, that all plants are not equally formed to sustain such a blaze of light; "some affect the sun and some the shade;" such as the pine-apple †, and orange, which require "a warm

* The admission of humidity into forcing-houses is attended by the most salutary consequences: it counteracts the bad effects of fire-heat, and is inimical to many insects. For this purpose, a steam-supplying apparatus is added to the best-constructed hot-houses, productive of the greatest advantages.

† It has long been observed by gardeners, that the pine-apple always does best in forcing-pits, merely from the circumstance of there being more shade.

shade;" and perhaps all plants which present a large reflecting surface of foliage to the sun, are content with a smaller share of his direct rays. These observations attended to, sun-shades may be applied for occasional use, and with the plants at a proper distance from the glass, will certainly secure them from all the inconvenience of such buildings, while none of the advantages are lost.

The kitchen-garden range of buildings includes pine-stoves, vineries, houses for peaches, and nectarines, figs, and cherries, hot-walls, pits for succession pines, melons, cucumbers; besides store pits for roots, tender vegetables, salading, &c., as well as frames for many purposes of cultivation. Mushrooms are usually raised in sheds behind the houses. The hot-houses are also used for growing early culinary vegetables, and small fruits in pots.

[To be continued.]

Chemical Manipulation, being Instructions to Students in Chemistry, on the Methods of performing Experiments of Demonstration, or of Research, with accuracy and success By Michael Faraday, F.R.S., &c.

We will not positively assert that no one except Mr. Faraday could have written this book, but we are of opinion that there are very few chemists adequate to such a task, which has manifestly required a considerable share of practical skill, much deep and theoretical knowledge, and no small degree of patience and perseverance, more especially shown in the clearness of the details, and the perspicuous manner in which he has managed to describe prolix and difficult processes. The work moreover fills up a chasm in chemical literature, by embodying almost all that is important relating to chemical manipulation scattered through the writings of others; while the author's extensive experience has enabled him to correct their faults, and to present the student and operator with many new and important facts and processes, by which the researches of the laboratory are most essentially facilitated.

Such is our general opinion of the treatise before us, and we are persuaded that those who are capable of appreciating its merits will agree in our decision; but it is not so easy to

substantiate our judgment by quotations, in consequence of the general didactic character of the book, and the mutual dependence and connexion of its different parts. We shall attempt, however, to give the general reader an outline of its contents, and point out such parts to the chemist as we conceive particularly useful and worthy attention.

The importance of readiness and dexterity in the performance of experiments has been duly estimated for more than a century. The writings of Black, Cavendish, Priestley, and especially Scheele, as opposed to those of their predecessors, show that they had acquired considerable facility in attaining, by simple and economical means, those ends which had before consumed much time and much expense in their accomplishment: but it is only of late years that the refinements of manipulation have been carried towards perfection; and the researches carried on in the laboratory of the Royal Institution have been not a little conducive to this improvement: to no one, however, is this part of the science more indebted than to Dr. Wollaston, whose skill in what may be called microscopic chemistry is consummate, and who has a host of humble but industrious imitators. So essential, indeed, is the attainment of correct methods of manipulation to the progress of chemical science, that many entire trains of research are exclusively dependent upon it for success. It is true that it must always be subordinate to genius and invention; yet the person who could only devise, without knowing how to perform, would comparatively be able to lend little aid to the extension and usefulness of knowledge: and were it not an invidious task, we might be able to show that some of the greatest discoveries and improvements of the science have originated in dexterity of experiment, rather than in profundity of design. By tact, therefore, in manipulation, a considerable advantage is gained, independent of that resulting from an acquaintance with the principles of the science; and this is so considerable, that, of two persons of equal talent and information in other respects, he who is the best manipulator will soon be in advance of the other; the one will draw just inferences with accuracy and rapidity, while the other will be lost in doubt, and often led into error. Mr. Faraday has pointed out several other cases of prominent advantage, arising from skilful manipulation, especially when very small quantities of matter are to be operated upon, and where accurate conclusions are of more than ordinary importance, as in testing for arsenic and other poisons on judicial occasions. When the substance under examination is rare,

as often happens, the facility of working with small quantities is also of much importance, as otherwise the opportunity of gaining information may be lost, or only retained at great expense. "There existed," says our author, "in the British Museum a small fragment of a black stone, the source and history of which was unknown: it was unique, no other specimen being in the Museum, or known to be in existence; yet as it presented some peculiar characters, Mr. Hatchett was induced to examine it, and, working with a portion of the stone weighing not more than two hundred grains, he was enabled to discover in it a new metal, which he distinguished, by its various characters, from all those previously known, and which he named Columbium. Ekeberg afterwards discovered a metal, which he named Tantalium, conceiving it to have been observed and distinguished for the first time by himself; but Dr. Wollaston, who examined it, and compared it with columbium, was able to identify it with that metal, although he had not more than five grains of the stone from the British Museum upon which to make his experiments."

In short, there can be but one opinion respecting the first-rate importance of expertness in manipulation, and neatness, dexterity, and efficacy of experimenting. These are the subjects to which the present volume is directed, and which will, therefore, form a valuable accompaniment to the more general and systematic works. They are discussed under the following general heads:—

The conveniences and requisites of a laboratory.
Chemical apparatus, and its uses.
The methods of performing chemical operations.
The facilities acquired by practice; and,
The causes which make experiments fail or succeed.

The description of a laboratory is followed by two long and well-written sections on the arts of weighing and measuring, in which the account of the methods of determining specific gravities, and of the general management of a delicate balance, are well deserving the student's attentive perusal: indeed, there are no operations which are more frequently performed in a slovenly and careless manner, than those in which scales and weights are concerned; and we should advise the tyro to sit down with his balance and this book before him, and practise the manipulations which it explains.

The fourth section, on the sources and management of heat, is devoted to the construction and management of different kinds of furnaces, lamps, blowpipes, thermometers, and pyrometers, and abounds in useful hints, and in the details of

practical information; and the same remark applies to the succeeding sections on comminution and solution—indeed, we were surprised at finding so much to be taught in regard to these very simple operations. The seventh, eighth, and ninth sections treat of distillation and sublimation, precipitation, and filtration. Here, and indeed throughout the work, the wood-cuts are particularly distinct and well executed. In the section on crystallization, the uses of that process are enumerated; and to this succeeds an account of evaporation. All these operations are extremely well investigated and described, both as to their principles and as to the most proper means of effecting them; a number of curious circumstances are pointed out, by which their results are influenced, and by which certainty and success may be insured.

The uses of coloured tests are explained and illustrated in the twelfth section. Of coloured liquids the author chiefly recommends the infusion of red cabbage; and as it is not only a very good test for private experiments but of excellent service to the public lecturer in rendering certain changes of composition visible to an audience, it may be worth while extracting the directions for preparing it.

"583. The only substance of the kind, perhaps, worth keeping in solution, is an acid infusion of red cabbage. For its preparation, one or more red cabbages should be cut into strips, and boiling water poured upon the pieces; a little dilute sulphuric acid is to be added, and the whole well stirred: it is then to be covered and kept hot as long as possible, or, if convenient, should be heated nearly to boiling, for an hour or two, in a copper or earthen vessel. The quantity of water to be added at first should be sufficient to cover the cabbage, and the sulphuric acid should be in the proportion of about half an ounce of strong oil of vitriol by measure to each good-sized plant. This being done, the fluid should be separated and drained off, and as much more hot water poured on as will cover the solid residue, adding a very little sulphuric acid. The whole is to be closed up, and suffered to stand until cold, and then the liquid poured off and added to the former infusion. The cabbage may now be thrown away. The infusion is to be evaporated to one half or one third its first bulk, poured into a jar, allowed to settle, and the clear red fluid decanted and preserved in bottles. The residue may have water added to it, the solid part be allowed to subside, the clear liquor drawn off, evaporated and added to the former, or it may be dismissed altogether. This solution will keep for a year. When re-

quired for use, the acid of a small portion of it should be neutralized by caustic potash, or soda, (not by ammonia,) when it will assume an intensely deep blue colour, and will, in most cases, require dilution with twelve or fourteen parts of water. The red liquor of pickle cabbage will, occasionally, answer the uses of the solution, and is, when required for service, to be neutralized in a similar manner."

For test-papers, litmus and turmeric are the most essential, and several precautions in preparing and using them are here pointed out, which, though apparently trivial, are, in fact, extremely important in insuring correct conclusions. We transcribe a part of the account of the applications of these coloured papers, as a specimen of the clear minuteness with which the details of the work are given, and as a sample of the author's general method and style, where subjects of much greater intricacy are to be explained.

"591. In using these test papers with a fluid suspected to contain free acid or alkali, or knowing that one of these substances is predominant, to ascertain which is so, all that is necessary is to moisten them with the liquid, and observe the change: if the fluid be acid, the blue colour of the litmus will immediately become red; if alkaline, the yellow colour of the turmeric will be changed to a brown. The moistening may be effected by dipping the paper into the liquid; but a better method is to touch the edge of the slip with a rod dipped in the fluid. In the latter case there is no risk of contamination to the fluid from the paper, and only a very minute quantity of the liquid is used at once.

"592. These trials must be made by day-light; artificial light not permitting that just estimation of the changes by which the presence of a small excess of acid or alkali is to be determined. As the proportion of free acid or alkali diminishes, the intensity of the new tint produced upon the paper is also diminished; and when in very small quantity, it requires considerable attention before a decision can be arrived at. The test paper should occasionally be touched with pure water in the immediate neighbourhood of the part where the solution has been applied, for any change in appearance that may have occurred, not due to mere moistening, is then readily perceived.

"593. Although acid is generally tested for by litmus paper, and alkali by turmeric paper, yet the former is sometimes used advantageously for the latter purpose, being first slightly reddened, either by exposure to the air, or by momentary contact with muriatic acid fumes. When the

paper thus modified is used to detect a free alkali, instead of turmeric paper, that substance is indicated by the restoration of the original blue colour. Litmus paper is best slightly reddened for this use, by putting a drop or two of muriatic acid into a large jar, allowing it to stand a few minutes, and then bringing the paper towards the mouth of the jar, or carefully placing it within: so soon as the blue tint has become slightly reddened, the paper should be removed for use. If too much acid be imparted to the paper, the delicacy of its indications is injured, because of the greater quantity of alkali required to neutralize the acid, and restore the blue colour. For the same reason a paper free from alkali or carbonate of lime has been recommended for the preparation of these tests: for these impurities, combining with a minute portion of acid, neutralize it, and thus prevent that delicacy of indication which the test paper ought and may be made to possess."

The mode of determining the value of alcaline substances, or "alcalimetry," is described at length in this section. Our readers, however, will here recollect that there is an error respecting the specific gravity of the acid, which Mr. Faraday has corrected at page 221 of the present volume of this Journal. The thirteenth section is allotted to crucible operations, and the fourteenth to furnace tube operations. They are full of minute and admirable instructions, evidently deduced from long experience, and detailed with the same precision and clearness which we have already eulogised. The fifteenth section, which occupies nearly a hundred pages, relates to "pneumatic manipulation, or management of gases." Every paragraph of the instructions here given will be found to contain something of importance to the student; it is, indeed, a valuable essay upon a difficult and nice department of chemical research.

Under the head "Tube Chemistry," in the sixteenth section, a variety of means are pointed out, of working with and employing glass-tubes, as substitutes for more expensive and formal apparatus. Indeed, the young chemist cannot do better than practise the art of bending, drawing out, and sealing tubes, as here directed, (and in the nineteenth section,) by which he will soon gain the requisite dexterity in forming them into test tubes, retorts, and so on, and be enabled to furnish his laboratory with a quantity of very useful vessels and apparatus, at a very moderate expense.

The application of electricity to chemical purposes forms the subject of the seventeenth section, in which the manage-

ment of electrical machines and apparatus is described, and the circumstances necessary to facilitate investigation and insure success are pointed out. To this succeed the management and composition of lutes, and a chapter on bending blowing, and cutting glass.

Cleanliness, order, and regularity are of the utmost importance in the laboratory; and though the appearance of the chemist himself is often such that he appears "to doat upon dirt," the strictest nicety must generally be observed in the state of his utensils and apparatus. These matters must, indeed, generally engage his personal attention; and it is not sufficient that glasses and other vessels be merely washed and wiped in the usual way, but they are generally required to be free from the minutest portions of adhering matter. A section is accordingly appropriated to the subject of cleanliness and cleansing, in which, and in that which follows it, entitled "General Rules for young Experimenters," much information is conveyed that will prove useful to those who are commencing the practice of experimental inquiries in chemistry, and also to such as, having made some progress, have indulged themselves in slovenly habits. Macquer's observations on this subject, as quoted by our author, are so much to the purpose, and so well deserving the serious attention of the young chemist, that we shall stand excused for inserting them in this place. He says, "A persuasion must exist that arrangement, order, and cleanliness, are essentially necessary in a chemical laboratory. Every vessel and utensil ought to be well cleansed as often it is used, and put again into its place; labels ought to be attached to all the substances, mixtures, and products of operations which are preserved in bottles or otherwise; these should be examined and cleansed from time to time, and the labels renewed when required. These cares, although they seem to be trifling, are, notwithstanding, the most fatiguing and tedious, but the most important, and often the least observed. When a person is keenly engaged, experiments succeed each other quickly; some seem nearly to decide the matter, and others suggest new ideas; he cannot but proceed to them immediately, and he is led from one to another; he thinks he shall easily know again the products of his first experiments, and therefore he does not take time to put them in order; he prosecutes with eagerness the experiments which he has last thought of, and in the mean time the vessels employed, the glasses and bottles filled, so accumulate that he cannot any longer distinguish them; or at least he is

uncertain concerning many of his former products. This evil is increased, if a new series of operations succeed, and occupy all the laboratory; or if he be obliged to quit the place for some time, every thing then goes into confusion. Hence it frequently happens that he loses the fruits of much labour, and that he must throw away almost all the products of his experiments.

"The only method of avoiding these inconveniences is to employ the cares and attentions above mentioned. It is indeed unpleasant and very difficult continually to stop in the midst of the most interesting researches, and to employ much valuable time in cleaning and arranging vessels and attaching labels. These employments are capable of cooling and retarding the progress of genius, and are tedious and disgusting; but they are nevertheless necessary. Those persons whose fortunes enable them to have an assistant operator, on whose accuracy and intelligence they can depend, avoid many of these disagreeable circumstances; but they ought nevertheless to attend to the execution of these things. We cannot depend too much on ourselves in these matters, however minute, on account of their consequences. This becomes even indispensable when the experiments are to be kept secret, at least for a time, which is very common and often necessary in chemistry.

"When new researches and inquiries are made, the mixtures, results, and products of all the operations ought to be kept a long time well ticketed and noted. It frequently happens that at the end of some time these things present very singular phenomena, which would never have been suspected. There are many beautiful discoveries in chemistry which were made in this manner, and certainly a much greater number which have been lost, because the products have been thrown away too hastily, or because they could not be recognised after the changes which happened to them."

The uses of equivalents, and the method of employing Dr. Wollaston's scale, form the subject of the twenty-second section of Mr. Faraday's book; and of the concluding sections, the twenty-third contains a quantity of miscellaneous remarks, and the twenty-fourth is appropriated to "a course of inductive and instructive practices;" that is, to a selection of minute instructions respecting the use of instruments, and the performance of operations.

Such is an outline of the contents of this volume, of which we have felt ourselves obliged to speak in terms unequivocally

favourable; in fact, it contains, strictly speaking, nothing to criticise. It is minute, laborious, and very unpretending, and contains a body of instructions for the performance of experiments, and of descriptions of the modes of managing and applying apparatus, which is not to be had elsewhere, being manifestly derived from diligent research, extensive experience, and correct judgment. It is not a book for amateurs, for they will presently learn from it that there is no royal road to the science of which it treats; but the real student, who will seriously follow its laborious details, will discover in them an acceptable and sure guide through the crooked and intricate, as well as the straight paths of chemistry. Those, however, and those only, who are well versed in the business of the laboratory, both as experimentalists and teachers, can duly appreciate the weighty service which Mr. Faraday has here performed.

Statistical Notices suggested by the actual State of the British Empire, as exhibited in the last Population Census. Communicated by Mr. Merritt.

[Read before the Literary and Philosophical Society of Liverpool.]

The population returns of the *decennial lustrum*, or period of ten years, which ended in 1821, were delayed for a considerable time, on account of the difficulties which have always occurred in taking the population of Ireland. They have now, however, been some time completed, and from the *data* they afford, a few reflections naturally present themselves, which though sufficiently obvious, yet, from the extreme interest of the subject, may be thought deserving of being brought together, and exhibited in a connected form. They point out some peculiarities in the situation of this country, which distinguish it from almost every other nation that has yet existed in ancient or modern times.

From the notices which have been published respecting the different districts, it may be inferred, that the portion which may be termed the *Urban* population, has augmented in a much greater degree than the ***Rural.*** The general ratio of increase has, however, been very great, and, in the opinion of Mr. Malthus, still continues at the same rate. That eminent

economist has lately given it as his opinion, before the Emigration Committee, that the present inhabitants of the British Islands do not amount to less than twenty-two millions and a half. This estimate is perhaps a little exaggerated; but as it may be assumed sufficiently near the truth for all the objects of general speculation, I shall proceed to point out a few of those leading peculiarities, to which I have just alluded. In the first place we may assert, I apprehend, on sufficient grounds, that Great Britain is the most populous nation which has existed since the Christian era. No other instance has occurred in which an extent of continuous surface of 93,000 square miles has sustained a population of twenty-two millions. Italy, which is not of much greater extent, has sometimes been rated at nearly the same amount, but this estimate has been formed in the absence of all actual enumeration, and is now ascertained to be a considerable exaggeration. No other part of the world can enter into the competition, unless it be certain districts of China and Japan, but which, as our knowledge of them in this respect is quite uncertain, I shall leave wholly out of the question. How far some nations of the ancient world may have approached or gone beyond us in the race of population, is perhaps equally lost in uncertainty. There is reason to believe, as I have endeavoured to demonstrate on another occasion, that some districts of the old world exceeded, in this respect, any country of modern ages. Amongst them, perhaps, may be reckoned Egypt, Mesopotamia, the lesser Asia, and some parts of Persia: but certainly, neither in ancient nor modern times do we find any instance of a single, compact, distinct empire, exactly defined, identically governed, and peopled by twenty-two millions of souls on the same extent of soil; this is undoubtedly a peculiarity the most striking which can exist among nations.

In the second place, we may, I think, affirm with tolerable certainty, that no nation ever contained so many large cities. On this point Great Britain exhibits a splendid superiority. We have two cities of the first class, London and Dublin; the one with a population of more than a million, the other with little less than three hundred thousand. Of cities of the second class, or those which reach one hundred thousand inhabitants,

or above that number, we have seven, *viz.*, four in England, Manchester, Liverpool, Birmingham, Bristol; two in Scotland, Edinburgh and Glasgow; and one in Ireland, the city of Cork. These seven average considerably more than one hundred thousand each. We have fourteen towns of the third class, or those containing from thirty to fifty thousand or upwards of inhabitants, *viz.*, ten in England: Portsmouth, Plymouth, Norwich, Leeds, Sheffield, Nottingham, Bath, Newcastle, Coventry, and Hull. Two in Scotland, Paisley and Dundee, and two in Ireland, Belfast and Limerick. Of towns of the fourth class, in which are usually reckoned those of from fifteen to thirty thousand inhabitants, we have at least thirty, and probably more. A slight glance at the principal nations of Europe, with this view, will show at once their immense inferiority.

To begin with France, the most populous of the great sovereignties. That empire possesses only one city of the first class, *viz.* Paris, which is inferior to London by one third. She has five of the second class, *viz.*, Lyons, Bourdeaux, Marseilles, Lisle and Rouen; but, according to the latest information which I have been able to obtain, they will not reach, by a very considerable proportion, the average number of the seven English cities of the same class. France has also eight towns of the third class, *viz.*, Amiens, Caen, Nantes, Brest, Toulouse, Toulon, Mentz, and Versailles. I am not quite sure, as no census has lately been taken, whether two or three of the following towns ought not to be included in this class, though I am inclined, on the whole, to a contrary opinion, *viz.*, Melun, Montpelier, Nanci, Dijon, Tours, Rennes, and Troyes; they will not, however, I am persuaded, come near the average of the British third-rate towns. The same remark will hold as to the number and size of the inferior towns.

With respect to the rest in rank of the great monarchies, the Austrian Empire, a very few words will suffice; as it cannot pretend to come into any competition with us, on the point in question. Austria possesses only one city of the first class, and three of the second, *viz.*, Vienna, Prague, Milan, Venice. The towns of the third rank are proportionably few. With Spain, Russia, and Prussia, it would be idle to enter into any comparison.

It must be confessed, however, that the present kingdom of the Netherlands, as established by the congress of Vienna contains, in proportion to its extent and population, more large towns than any single state which now exists, or perhaps has ever existed. With an extent of territory and number of inhabitants scarcely exceeding one-fourth of the British dominions, that kingdom has one city of the first class, Amsterdam; two of the second rank, Rotterdam and Brussels; and probably as many of the third class as Great Britain herself. But the Kingdom of the Netherlands is in itself too insignificant to enter into any competition with such a state as Great Britain for any objects of general comparison. The various states comprehended under the common geographical appellation of Italy, if that superb country was united under one head, is the only one of the European nations which, under the view we are now considering, could sustain any parallel with Great Britain. But this union, so desirable in many points of view, would probably diminish its pretensions as a nation of large cities. Many of these have reached their present grandeur and extent by having been long the seat of a court or a government, and would perhaps decline considerably if reduced to the rank of mean provincial capitals. But even under any circumstances of territorial union, Italy could not be held to comprize more than one city of the first class, *viz.*, Naples, and six of the second, *viz.*, Turin, Milan, Venice, Genoa, Florence, and Rome; whereas, as we have just seen, Britain has two of the first and seven of the second, and these superior in size and number of inhabitants.

The third peculiarity which I have to remark in the actual situation of the British dominions is, that no nation ever had so great an *urban* population, or so large a proportion of its inhabitants residing in towns. This peculiarity is intimately connected with that which I have just described; but it is nevertheless a very different characteristic. Great Britain is not only distinguished for the number and size of her large cities, but for having so *great* a number of them on so *small* a territory. By the census of 1811, it was found that nearly half our population resided in towns, and at present, I apprehend, the proportion will be found still greater. In this

respect no nation has ever approached us. The French economists were of opinion that not more than one-fourth of the people of France lived in towns; and the later statists, who have alluded to the subject, contend that a still greater proportion of the population is rural. This will not appear exaggerated when it is recollected that all the lower classes of that country subsist principally on vegetable food, and that, consequently, the greater part of the soil being under tillage, a great number of hands is required for its cultivation. In Great Britain, on the other hand, the inhabitants of all classes consume a great quantity of animal food, and, of course, a great part of our lands, being in a pastoral state, require a small proportion of occupants. In the kingdom of the Netherlands, it is supposed about one-third of the inhabitants live in towns: in Italy about one-fifth: in Austria, Spain, and Russia, except the province of Siberia, where the abundance of manufactures congregates the people in masses, not more than one-fifth. In Russia, Sweden, and Norway, where, amongst the lower classes, nearly every family is its own manufacturer, not more than one-eighth or one-ninth.

The fourth and last of these peculiar characteristics which I shall remark, is, that no great nation ever employed so large a proportion of its people in trade and manufactures. In speaking thus, I leave out of the question the Italian and Flemish republics of the middle ages, and the Hanse Towns, free cities, and United Provinces of later times. I speak only of great and extensive countries. It will appear, I doubt not, by the present census, that at least half our whole population is employed in trade, commerce, or manufactures. This is a feature altogether singular; a circumstance to which no parallel can be found in the ancient or modern world.

From these premises, a few observations, in the way of corollaries, will naturally suggest themselves.

In the first place, such a state of things is indicative of great wealth and power. A country thus situated is, beyond any other, powerful for attack and strong for defence. A profusion of great cities can only be produced by extensive trade, and can only be maintained by a highly cultivated soil. The wealth acquired by the industry of the towns, reacts on the

industry of the agriculturist, and it is in this that the rea advantages of commerce primarily consist. In this way an extensive population is gradually generated, for no maxim or political economy is now more generally admitted, than that population is sure to follow close and to press hard against the means of subsistence. An affluence of inhabitants on a comparatively small territory, is itself the primary ingredient of power, and this first requisite of strength is, in the case of Great Britain, essentially corroborated by our insular situation. Surrounded by dangerous coasts and tempestuous seas, we can only be approached at certain points and certain times; whilst, on the other hand, as this state of things supposes and supports a powerful navy, we are able in a great degree to choose our point of attack.

From a population such as we have described, of which only a very limited part is employed in creating the means of actual subsistence, a very considerable portion may always be abstracted for purposes of attack or defence. It is usually calculated, that one-fifth part of the inhabitants of every country is capable of bearing arms. On this calculation, Great Britain contains four millions of fighting men, of whom it is believed one million might be formed into an army without any very serious interruption to the essential operations of agriculture and commerce. This supposition may seem a little extravagant, but it must be recollected that, at one period during the late war, the number of men under arms was actually calculated at seven hundred and fifty thousand.

In the second place, such a state of things is favourable to public liberty. The congregation of men in great masses is found to give great force to the influence of public opinion; by the spirit of discussion which it generates; by the anxiety for intelligence which it diffuses; by the collisions of opinion which it engenders, and by the facility of union which it affords. Nations purely or principally agricultural are generally under a despotic government, especially large states, for the maxim of *divide et impera* is applicable as well to internal as to external politics. Ancient Persia and Assyria, and modern Russia and Poland, are instances in point. The fierce and demoralizing tyranny of the feudal system, which, after

the destruction of the Roman monarchy, left scarcely any other division of the people than those of tyrant and vassal, could only be effectually broken by the rise of great towns. These communities were alone competent to resist the aristocratical and subordinate despotisms into which all the nations of Europe were subdivided, and which, as is well known, overawed the throne, whilst they enslaved the people. In confirmation of this, it may be remarked, that the free republics of antiquity, as well as those of the middle ages, derived the spirit which nurtured them almost entirely from the capital city; and though, in the former case, there was scarcely any commerce to excite the activity of the people, yet the mere congregation of a numerous body of men sustained the power of public opinion.

But the most important question remains behind. Is a civil community thus constituted favourable to individual virtue and happiness? This is assuredly the point which it most behoves us to ascertain, since no truism is more obvious than that power and opulence, and refinement and splendour, and even liberty itself, are only so far valuable as they tend to make men wiser, and better, and happier. Is it true, then, that Great Britain has anteceded other nations in these fundamental points, as much as in those we have just described? This question cannot be answered without some hesitation: for we may say, with Addison's facetious Knight, "that a great deal may be urged on both sides." On the one hand it is certain that our situation is eminently favourable to intellectual improvement. The increasing spread of instruction, and the rapid advancement of knowledge which are necessarily concurrent with our career of prosperity, must ultimately advance us in the scale of moral and rational agents. If knowledge be power, it is also happiness; for communities as well as individuals would all be happy if they knew how to be so. It is also certain that the incessant struggles of competition and the strenuous efforts for distinction which are always at work in an over-peopled and highly refined country are favourable to the active virtues. They operate amongst the higher classes to provide many objects of laudable ambition; and amongst the lower, afford perpetual facilities for bettering their condition,

and furnish an incessant supply of occupation, the want of which is sure to open the door to the incursion of all the worst propensities and basest vices. They bring into action all the resources of human ingenuity; all the aids of fortitude and enterprise; all the trials of patience and perseverance; all the equanimity demanded by the constant mutations and rotations of fortune. It is not to be denied, moreover, that the first-rate virtues of beneficence, charity, and hospitality, take root and flourish with peculiar vigour in a commercial community. The fluctuations of condition to which almost every man knows himself liable, and the constant proximity of distress and opulence, offer perpetual excitements to the benevolent affections.

These, it must be confessed, are important ingredients in the composition of human happiness; but considerations not less momentous present themselves on the opposite side, for every thing in human affairs is on a system of compensations. It is not to be denied that a state of society, in which one-half of the population is congregated in towns, and nearly a moiety of this half crowded together in enormous factories, is highly unpropitious to virtue, to health, and to happiness. In these huge receptacles of human labour, it would be absurd to expect that the women should be distinguished for their modesty and propriety, or the men for their prudence, temperance, and regularity. It is an unhappy law of human nature, that the force of example is most prevalent on the side of vice. A few depraved characters scattered amongst a multitude are commonly found sufficient to corrupt the whole mass: hence we may always expect to find, in the seat of a great manufactory, all the worst ingredients of civilized society; all the base depravities of a luxurious and opulent community, combined with much of the grossness and rudeness of the savage state: in a word, all the corruptions of high civilization without any of its polish. Nor is this mode of life, generally speaking, more favourable to health and comfort than to good morals. The constitution of the young is impaired, and their growth retarded by excessive labour and close confinement. Those of maturer age are glad to seek relief from the depressing effects of a wearisome and monotonous labour, unwholesome air, and constant

restraint, in intemperate indulgence; and all the long train of vices and miseries to which the poor are liable, follows of course. Nor are their prospects for the future often such as to encourage hope or stimulate exertion. The habitual improvidence of the poor is aggravated in their case by the dangerous fluctuation of their trade. Sometimes they are eagerly courted with high wages, and lavish promises; at others, no employment is to be had, and not enough can be earned, even by the most unnatural exertions, to sustain their families. Nothing can be imagined more fatal to order, regularity, and comfort, than these vicissitudes. Hence it commonly happens, that, in the decline of life, these poor creatures are driven to the sad resource of parish relief. It is moreover not one of the least evils of the manufacturing system, that it has a tendency, in prosperous times, to generate an excessive population, which, on any great reverse, is suddenly thrown on the community as a superfluous burden. The changes of a fashion, the caprice of public taste, or the sudden interruption of a foreign market, will reduce thousands to helpless and unexpected poverty.

It must, however, be admitted, that the picture of rural life has also its unfavourable aspect. Those who retire into the country are apt to find themselves somewhat disappointed in their expectations of rustic simplicity and pastoral innocence. In situations where every breath of air, and every feature of nature express nothing but peace and love, they are a little surprised to see the selfish and malignant passions at work in all their baneful activity; to find, as in the purlieus of a court, the symptoms of "envy, hatred, malice, and all uncharitableness." Still we shall find that instances of utter depravity and abandoned profligacy are of much rarer occurrence than in great towns. In a village, every individual is known, and the very consciousness of being conspicuous, creates a sense of shame which is highly salutary. It has often been observed, that men in a body will commit, and even justify, atrocities which no individual amongst them would be capable of attempting, if not screened by the shelter of a crowd. We find, accordingly, in the annals of Wesley and Whitfield, that the great scenes of their operations are in collieries, factories,

mines, canals, and all the other appendages of a great commercial and manufacturing nation. It was there, according to Whitfield, that the "Arch Enemy" raised his triumphant standard; it was there that the harvest of lost souls was ripe and abundant. But the most decisive proof of the comparative purity of the rural population above that of the manufacturing districts, is the fact that the single town of Manchester will furnish ten times more criminal prosecutions than two Welch counties which contain an equal number of inhabitants.

On the whole, I think we cannot escape the conclusion, that, though a certain degree of commercial and manufacturing property is necessary to stimulate the agriculture of a nation, and to call forth its utmost powers of production, yet that it is not desirable that this country should proceed much further in that dangerous career, or increase still further the disproportion between its urban and rural population. The late increase in our numbers is so rapid and alarming, that I am afraid some positive checks (to use Mr. Malthus's language) of very terrible potency must soon be brought into action. The forcible lines of Goldsmith, though that great poet knew little enough of political economy, are applicable to the wise and benevolent statesmen of all times—

> 'Tis theirs to judge, how wide the limits stand
> Between a splendid and a happy land.

On the Modern Ornaments of Architecture, &c.

In no age since the Augustan era of Rome, perhaps, has decoration of the interior of dwellings been carried to greater excess than at present; nor, since the days of the florid style of Gothic architecture, has the exterior received more embellishment. Architectural ornaments have generally been copied from the antique, those especially which belong to the orders. Indeed there is a kind of classical standard, which governs the architect in the execution of public edifices, from which he cannot with propriety depart. National, and regal emblems, wherever suitable, should always be introduced in public buildings, and in those of a private or mixed character

all legitimate ornaments may be displayed. Of this class the acanthus, vignette, the branches of the olive, and leaves of the palm, the crown of laurel, the chaplet of myrtle, and the wreath of roses, are all proper when judiciously introduced; and the rose and honeysuckle flowers, and the folicles, trefoils, cinquefoils, &c., which so often occur on sculpture and plaster work. are also all proper, because they are imitations of nature.

But in our present style of decorative execution, from the most elaborate finishing of a regal palace, down to the pattern of a milk-maid's gown, there is such latitude taken in the display of licentious fancy, that imagination itself is baffled to find anything in the infinite variety of nature's works, to which their designs can be compared, or to which they bear the most distant resemblance!

It is really unaccountable, that the whole tribe of our artists, the ornamental statuary, scagliolist, paper-stainer, weaver, chintz and cotton printer, &c. should all be "straining their low thought to form unreal" forms and figures; and striking out designs the most intricate and complicated, to the utter neglect (except in very few instances) of those numberless simple though transcendently beautiful configurations, which everywhere appear in the works of nature.

This is surely a dereliction of all propriety, an exuberance of grovelling taste which no consideration can excuse, nor reason justify. In this age of refinement, good taste should be the guide and handmaid in all things where invention is necessary, and design requisite; whatever is grotesque or fantastic, should be banished from our labours of art, and the elegant forms of vegetable or animal nature alone take their place.

If it be asked, how it happens that such obliquity of fancy (for it cannot be called taste) should so generally prevail, the answer is, were these pattern-mongers to copy from nature every body could judge of their ability as imitators, and, if unfaithful, would decry the artist; whereas, whilst bringing forth his nondescript and nondescribable forms of imaginary figures, he escapes the lash of the critic, which otherwise he would be subjected to,

It may be granted, that it is as ridiculous to form stone or plaster flowers, as those geometrical frets and fanciful nothings which are usually pourtrayed in architectural decoration: but it may be answered that if any ornament be necessary, that of a nondescript character is not more appropriate, as such, than natural forms would be; and these latter having a name, and many of them an emblematical character, may be often applied with a propriety which cannot belong to the other.

The old fashioned tapestry, notwithstanding its sombre appearance, was in its plan much more rational than the multifigurations of our modern paper hangings. The first represented some historical event or legendary tale, yielding some mental information, or it taught perhaps a moral lesson—the eye was amused while tracing the ideas of the ingenious sempstress; but in our ephemeral and gaudy ten-thousand-times repeated *paper* nothings, there is no design to interest, nor combination to amuse the eye longer than a transient glance. Even the Chinese, who, in all their decorative finishings shew *rigidity* itself, have escaped from tame mannerism in paper hangings, by imitating, from the edge of the carpet to the ceiling, all the gradations of turf, herbs, shrubs, and trees, upon a sky ground, enriched with figures or rather portraits of flowers and fruit, as well as beasts, birds and insects. This though it cannot deceive the spectator for one moment in mistaking a fictitious for a real scene, yet is certainly far superior to European paper-upholstery, as it at least may introduce a knowledge of natural history, which the latter has no pretension to, indeed seems studiously to discard, as beneath imitation.

All this vitiated taste, or fashion rather, is to be regretted; especially as it appears that those

> Fancied forms which on the ceilings sprawl,
> And shapeless frets which decorate the wall,

are just as expensive, and difficult of execution, as the most elegant imitation of vegetable or animal configuration would be; and surely when such variety of forms are presented to the artist, they deserve to be copied as transcendently superior to the capricious fancies of the most celebrated decorator, or of

the most splendid fashionable designs, either in the works of the sculptor, scagliolist, &c. or the more insignificant designers of figured paper or drapery. Indeed there can be no good reason why ox-heads and garlands (now the days of sacrifices are past) should not be banished from the frieze and entablature, to admit the far more appropriate figures of foliage, fruit, and flowers, aquatic as well as terrestrial, which every garden yields;—and for interior enrichments of cornices, mouldings, &c., the curious and elegant forms of the testacea, would afford beautiful copies for imitation.

In fine, if there be any merit or propriety in the adaptation of whatever is elegant in form, beautiful in outline, harmonious in tint and proportion, and congruous in combination, such may readily be found in the animal and vegetable kingdoms. Faithful representations of such objects, not only open a fine field for the exercise of individual ability, at this time, but also a source from which might be drawn a large share of public patronage, and consequent commensurate reward. Indeed it is now pretty evident that in many things, especially in the minor works of art, we have been too long and too rigidly impressed with a veneration for the works of antiquity, or what is equally benumbing, a passive following of tyrant fashion; and that many a bright genius has been "nipped in the bud," and remained "twinkling in the socket" of Grecian and Roman rules, who, if venturous enough to have burst the shackles of professional thraldom, would have improved and elevated his art, as well as himself, by designs and works which would have advanced his profession and adorned his country.

But it is not yet too late; a knowledge and study of the genuine elements of taste, whether of art or nature, and a mind embued with rational perceptions of all that is beautiful and picturesque, and grand or sublime in either, will rise superior to all precedential fetters, as well as all modern mannerism, and will equally regard the excellencies of the ancients, as it will avoid the errors of some modern artists, who, in leaving the beaten track, have deviated far and widely from the point to which good sense and good taste would have led.

A list of plants, &c. which exemplify all that is elegant in form, beautiful in outline, and graceful in position, should have accompanied the above imperfect remarks, but this must be deferred to another opportunity.

J. M.

De l'Influence des Agens Physiques sur la Vie. *Par* W. F. Edwards, D.M., &c.

[Continued from the last Number.]

In our last number we presented our readers with a general abstract of the first part of this valuable work. The second part refers to animals of the *cold blood* order, including *fish* and *reptiles.* The *larvæ* of the latter underwent some comparative experiments detailed in the first chapter, because they partake of the nature both of *fish* and *reptiles*, as to their *respiratory function;* the imperfection of their intermediate state and developement of organization not interfering with the objects in view, and the double mode of aërification being exercised unequally. The *skin* of these young animals furnishes them with the means of producing the requisite changes in the blood by *absorption*, as in the adult, while it lives in water; and the cutaneous respiration goes on through this medium at a temperature which the subsequent more perfect animal is unable to endure. The object entertained is the influence of physical agents upon the changes which these animals pass through in their form and structure.

An important condition of their advancement to maturity seems to be, that the nutriment suspended in the water should be in very small and limited proportions. Temperature also influences their constitutional changes.

Sometimes the *larvæ* pass through the winter in their primitive state; a fact not generally known. Some *tadpoles* were confined within wooden boxes submersed in the river Seine, in which holes were perforated to allow the stream to pass through, without the possibility of the animals' rising to the surface of the water, and thus to inhale air. Others were placed in a large vessel of Seine water renewed at intervals, with power to rise above the surface. Ten in twelve of the first box underwent no transformation, the others having gone partially through their change. But

those of the large vessel, and not submersed in the river, passed through their changes of form without the least appearance of the phenomenon being retarded. The running waters of the Seine probably contained nutritious matter, which the water periodically renewed was more deficient in.

Under circumstances of moderate nourishment and temperature, the tadpoles under water did not complete their changes but in a very partial and protracted manner, while the greater portion made no change. The great difference in the circumstances of the experiments seems to have been the access to the air of those which went through their transformations as usual. Exclusion from light made no difference in the results, and these were solely influenced by occasional renewal of air from pulmonary respiration.

These animals possessing a double respiration, cutaneous and pulmonic, that is, absorbing air from the water around them and inhaling it from the atmosphere on its surface, renders these facts highly curious. Fish possess only the means of aquatic respiration, and the influence of temperature was tried upon them submersed in water deprived of its air by previous boiling, the heat being varied from 0° to 40°. The fish died quicker under these circumstances than the frog species in the same situation; but their lives were prolonged more in the *descent* of the thermometer than during its *elevation*, as also occurred with the experiments on frogs and salamanders; and, in both cases, the younger the animal, the less it could resist the higher temperatures. At 40° the young animals only survived about two minutes, and the adults many more.

Fish were also submersed in closed vessels of aërated water, and, by varying the temperature and the quantities of water, the duration of their lives was augmented in proportion to the increased volume of the liquid, the temperature remaining the same; but when these experiments were conducted in open vessels, the contact with the atmosphere altered the phenomena. At 20°, a small fish expired in four hours; and when the temperature was lowered to 10° or 12°, the same sort of animal lived several days; and when the water was kept clean by being changed every twenty-four hours, the fish lived indefinitely.

It is known that fish rise periodically to the surface of the waters to respire; and Dr. Edwards discovered that they did so when they have reduced the properties of the air dis-

solved in the water to a lower standard than is requisite for the proper aërification of their blood, thus renewing their supply of *oxygen*.

The functions of this class of animals have always been obscure, and their phenomena are different from those of others. Different species of fish die at various periods when deprived of water, some in a few minutes, others in a few hours; and it appears that their dissolution arises not so much from incapability of atmospheric respiration (for the experiments of Sylvester prove that they can respire pure air), as from the different state of the air.

Some experiments on *lizards*, *snakes*, and *turtles* conclude the researches among cold-blooded animals. The skins of these, like those of the frogs and salamanders, received a vivifying influence from the air, mainly acting, in conjunction with pulmonary respiration, to promote their existence. *Snakes* and *turtle*, their pulmonary respiration being insulated, from their skins being guarded from atmospheric influence, were found alive; but the *lizards* died in a few hours, when the vivifying contact of the air was removed from their bodies, and they breathed only by their mouths. Animals naturally defended by *scales* transpire much less than such as have their skins free. Thus *frogs*, *toads*, and *salamanders* yielded more by perspiration than *lizards*, *snakes*, and *turtle*, in a given time; and the porosity of the skin of course regulates the facility of transpiration in all cases.

With these experiments and remarks, Dr. Edwards concludes the second part of his researches. The third part includes *animals of warm blood*, in which will be found some curious and interesting remarks on the heat of young animals compared with that of adults.

Dr. Edwards refutes the common notion of young animals being necessarily hotter than adults. The heat of young puppies was very near that of the parent, or one or two degrees less, but this variation was not constant. Some new-born kittens and rabbits were also subjected to similar trials, and the results led to a conclusion that the temperature of young animals is *less* than that of adults.

According to these experiments, the power of resisting the cooling influence of the air acquires force as the animal grows up; and those examples related, in which artificial covering was adopted, show that nudity is not the only cause of the reduction of heat, which is, in fact, more referrible

to their infantile constitution. At first the sucking animal shows little variation from the parent temperature; then this becomes more and more reduced, and about the fifteenth day it is a degree or two below the mother.

Birds, which are warmer than mammiferæ, were next made the objects of experimental inquiry, and the young recently hatched exhibited a *lower* temperature than the grown birds. After removal from the shelter of their nests into a mild atmosphere of 17°, in one hour they cooled down from 36° to 19°, thus losing 17° in an hour. At an elevation of 22° the same results were obtained, and they cooled down to within one degree of the surrounding air. The plumage of birds has little if any influence upon their temperature. The production of heat lies within, and not on the surface of the animal; and if it be strongly developed, the removal of natural coverings does not influence the heat produced; and if it be weak, their addition will not prevent cooling. Birds recently escaped from the shell cooled to within two degrees of the air, whereas the unplumed adult birds scarcely lost one degree.

The distinctive character of warm-blooded animals to preserve an uniformity of heat has no reference to *bulk*. The *eagle* maintains the same temperature as the *wren* or the *tom-tit*, taking them at the same age, and placing them under the same circumstances; but if cooling measures be adopted, the lesser body parts with its heat faster than the larger, though ultimately they arrive at the same point. The dimensions of animals are infinitely varied; but the giant reaches no higher standard than the dwarf, nor sinks to a lower temperature.

In estimating the temperature of young animals, it must be taken into account that they are born at different periods of organic developement. Some come earlier into the world than others, and some are more perfectly formed than others at their birth, and more capable of helping themselves. This variation produces a different standard of heat after birth, and especially creates a variety of temperature among birds when tested at the same epochs of their existence. The season in which animals are produced also modifies their temperature.

The influence of age in modifying temperature is common both to mammiferæ and birds. Young and healthy sucking pigs cooled faster than their parent, their generating means of heat being more feeble. Animals of warm blood possess

the power of supplying heat at its *maximum* when first born; they then part with it by degrees, and, as they advance in age, their heat becomes gradually augmented again till it reaches the adult standard.

Dr. Edwards next proceeds to discuss the phenomena of animal temperature more exclusively regarding adults, and especially among those singular creatures of the mammiferæ which form an exception to the general law of nature respecting the uniformity of temperature as to warm-blooded animals. These beings are what are termed *hybernants*, such as the *dormouse*, the *hedgehog*, the *bat*, the *marmot*, &c., natives of Europe, which remain dormant during winter without any external signs of life and motion. The change which these undergo reduces them from the state of warm-blooded animals to that of cold. Unlike the rest of their class, the autumnal season lowers their temperature by degrees, till in winter it reaches so low as to be scarcely higher than the surrounding air. Their powers fail gradually, and their losses of heat are not repaired, till at length their respirations become slow and feeble, and the heart languidly urges the blood through the arteries. In this state there is an imperfect aërification of the blood, and a partial state of asphyxia, producing continued repose of the nervous and muscular system. But the temperature of these animals sinks no lower than the air, and remains sufficient to maintain a passive existence, till the returning spring raises their heat again, and they become lively and active till autumn; but even in spring these animals are characterised by producing less heat than others of their class.

If we seek to know the cause of this curious variety, we can only refer it to peculiarity of constitution, which is instituted by nature as adapted to animals placed in situations of rigorous cold, and where they cannot procure sustenance but in spring and summer.

Our author imitated the process of hybernation by artificial cold, and produced the same effects; and when he restored animation by gradual warmth, he found the animals as lively as before.

John Hunter and others have written on the natural history of hybernants, and Dr. Edwards regards only their temperature. The experiments on hybernants by artificial cold prove this fact, that hybernation is attributable to other causes than to the reduction and deprivation of nutriment; for the animals submitted to the ordeal of cold were well

fed, and in the lively season of advanced spring. The deprivation of food seems to be a local consequence provided for by the phenomenon of hybernation, and not its exciting cause. Nor does there appear to be any change of organization in these cases, but a state of constitution exists which we are unable to account for further.

We have, in the next place, a series of experiments showing the influence of the *seasons* upon animal temperature with the warm-blooded; by which it seems that they produce a variety of results: and it is demonstrated that animals of warm blood in general undergo some constitutional changes with the periodical returns of the seasons. When, for example, the highest degree of temperature is attained, animals no longer produce heat; so that their temperature continues below that of the air in the hot season. And, in the cold season, if the cold be not too rigorous, the animal's age offers a proportionate resistance to the cooling effects of the air as the approach to maturity is attained. An elevated and a depressed temperature thus produce contrary effects upon the internal powers of generating animal heat, a high temperature arresting them, and a low one promoting them. *Thus we cannot fail to observe the beautiful adaptation of means to final causes.*

Upon the subject of *asphyxia* in warm-blooded animals, Dr. Edwards found a great dependence between animal heat and the faculty of living without contact with the air, a state in which the blood is not aërated by respiration, and which is sustained by hybernants while in the dormant condition. Having submersed animals in water of various temperatures successively, so as to bring them under the influence of variable temperature, he found the *descending* scale of temperature the most hurtful. The *ascending* heat was that which prolonged life most. Between 20° and 10° the results were similar to those between 20° and 40°.

Animals, then, of warm blood in a state of asphyxia hold their existence on two principal conditions relative to heat; one regarding the different measures by which some develope their heat, and the other the degree of external temperature. The first is proper to animals naturally, the second fortuitous.

Upon the *respiration* of both young and adult animals the author arrives at a conclusion opposite to that of common opinion, which is founded on the notion of the heat in young animals being higher than that of the matured. Finding,

however, as already noticed, that the parent exceeds the temperature of its offspring after birth, it is naturally concluded that its consumption of air is also greatest. This was experimentally confirmed, and is in unison with other facts. In the first part of this work the vertebratæ of cold blood were also found to consume least air in proportion to their diminution of temperature. Temperature seems to act uniformly with all the vertebratæ, and their consumption of air is in proportion. The *mammiferæ* have a lower temperature than *birds*, and they consume less air than the latter. Fish and reptiles consume less air than the warm-blooded, and possess a lower temperature.

The influence of the *seasons* upon *respiration* is considered in the sixth chapter. Many changes occur in the atmosphere during the revolutions of the seasons, varieties in the temperature, and the pressure and density of the air. Dr. Edwards shows that the faculty of producing heat with warm-blooded animals is greater in *winter* than in *summer*, the constitution of animals being adapted to their individual climates; and in reference to the relation of this faculty to the consumption of air, it is presumable, all other circumstances being alike, that the consumption ought to be increased with the faculty of developing heat, and the experiments justify the presumption.

Upon the subject of *transpiration*, it is shown that the air not only exercises a vivifying effect upon the constitution, but one little less important in removing a vaporous substance from the surface of the body, and which is separated from the fluids before its conversion into vapour, and known by the name of perspiration or sweat, which transpires from the skin. The variations in the temperature of the air possess great influence over this function. Experiments on this subject were detailed most fully in our last Number, relative to cold-blooded animals; and therefore these need not now be repeated in respect to the warm-blooded, for the results are exactly similar, as to transpiration in equal and successive periods, the comparative influence of dry and moist states of the air, and the effects of air in motion and in repose. Inspection of the table annexed to the work displays the similarity of the effects produced by the same physical agents upon cold and warm blooded animals, and this accordance serves to afford mutual support to the different investigations.

We are now arrived at the fourth and last part of this

work. Much, however, of this part appertains to what has been already detailed upon other animals. But the modifications of heat in the human being, from the period of birth to maturity, will be found highly interesting. They accord precisely with the results obtained among the lower animals and mammiferæ, and present analogical proofs of the general application of principles laid down in the preceding portions of our notices.

While, however, we trace analogy throughout the animal kingdom, it must be remembered that there are infinite sources of variation arising from the extensive variety of species modifying those principles, which are governed by a general harmony of effect. Of all animals, man exhibits this variety the most, possessing, as he does, attributes above all the groups of his class, from his intellectual properties, speech, &c., rendering his race unique and superior to all others. Our curiosity cannot, therefore, be allowed to rest satisfied with the general application of principles, until we have observed their modifications in the human being as well as in brutes. It is highly interesting to inquire into the conditions of human phenomena, and examine the forces which man opposes in his intelligent character to the physical agents around him. He is equally liable to their influence, exists by their contact, and yields, like other members of the animal kingdom, to their destructive tendency. The essential distinctions appertaining to his economy are thus the more necessary to be understood. His *organization* affords him no shelter from the operations of physical laws beyond that of brutes; but the superiority of his nature may be supposed to modify their influence from causes referrible to his *sensibility*. These have formed the subject of Dr. Edwards's inquiry.

Man's state and condition, at his birth, place him in very different circumstances from those at which he subsequently arrives. Here, therefore, we see an extensive field of inquiry; and it is suggested whether, in the infantile state, man generates *less heat* than in more matured existence. Dr. Edwards has shown that the young of mammiferæ generally, being born at the period when their eyes are open, produce *less* heat than adults. It is, therefore, presumable that the generating powers of heat differ in the two states of existence which man goes through, the infantile and mature.

But the power of producing heat differs among adult animals, and it is desirable to know the limits of this faculty,

Moreover, this power differs in different parts of the body; so that, when experiments are made, we should always apply the thermometer to the same part of the body. Among twenty adult persons, Dr. Edwards found the average temperature 36°.12: in infants from a few hours to two days old, 34°.75 was the average. Thus we perceive that the temperature of human infants is *inferior* to that of adults. In infants born previous to the usual period, two or three hours after birth their heat was at 32° of Reaumur's scale. So far we perceive a similarity in man to the mammiferæ in general."

We have next a chapter on the effects of *cold* upon mortality at different ages. It is highly interesting to observe the care of animals towards their offspring, in protecting them against the effects of cold instinctively, at a period before their own powers of generating heat enable them to resist its baneful tendency.

Dr. Edwards endeavours to investigate the subject of cold, so as to discover its limit of action. He examined the young of mammiferæ and birds, the former born with closed eyes, and the latter unfledged. He exposed them separately and apart to the air, so as to be independent of each other's warmth, and they exhibited a temperature below their natural standard at the period of birth, even when a degree of artificial heat was applied beyond that of adult birds. The final result of these experiments was, that the application of heat may be conducive to their developement, but is not indispensable to their preservation. The author discovered, that the diminution of temperature is not equally injurious at all ages. The *younger* the animal, the *less* is the injury sustained by cold, because the faculty of producing heat is less powerful with the young than with the matured animal, the power increasing as the animal grows, and also with the increase of cold.

Still, however, this subject is open to inquiry, for the great variety of species, and other circumstances belonging to the animal creation, so modify the phenomena as to create an almost endless field of investigation. When warm-blooded animals are exposed by their parents to the atmospheric influence at an early age, they are better provided against the perils of cold, being born with an abundant source of heat. But, if the cold exceeds their powers of generating heat, the mortality is so much readier. Hence arises the danger of animals being born in the winter season.

Two circumstances are distinguishable, the refrigeration of the body, and the temperature it is capable of sustaining. The cooling is so much less injurious with the young. If two young animals of the same species be cooled down equally, the youngest suffers the least. But, in order to lower to the same number of degrees the temperature of bodies of different ages, the external heat should be lowered in proportion to the advancement of the animal towards maturity, in order to compensate for the difference which the modification of age produces.

While it is true that the younger animals suffer least from cold, it is, at the same time, to be considered that they cool down more rapidly. On this principle depends the mortality of our domestic fowls and other animals, the management of which requires so much observation and experience in order to rear them. In regions where the temperature is liable to great alterations in the course of the year, man and other vertebrated animals of warm blood are liable to suffer in their health; for, though *cold* should produce the resistance derived from the necessary constitutional developement of heat, this increase of caloric, having its limits, often exposes the constitution to the effects of too great reduction of temperature, as is exemplified in the frozen regions of the North Pole, in Siberia, and in Russia.

The young of mammiferæ, in general, were found by Dr. Edwards to differ very materially in the duration of their lives, in a state of asphyxia, often being limited to from five to eleven minutes, according to their developement at birth, the most advanced in organization living the longest period. The author proved these facts by placing animals in a state of asphyxia under water; and it is remarkable that, in all his experiments, the *voluntary* motions were always first destroyed, the *involuntary* outliving them. With dogs, cats, and rabbits, sensibility existed only three or four minutes. A puppy showed *automatic* signs of life nearly half an hour. The best divers appear to be able to remain under water from three to four minutes.

When animals are entirely deprived of aërial contact, it may be inquired, what are the principal functions exercised? When the air circulates through the lungs, it imparts to the blood a peculiar quality, by which its colour becomes changed. Deprived of this influence from the air, the blood acquires a dark colour, and the *nervous function* is simul-

taneously affected. Among *reptiles*, Dr. Edwards found that life could be maintained by this dark blood; but it is questionable whether the circulation of venous or dark blood can promote life in animals of the warm-blooded kind. Temperature certainly modifies their capability of existence. Under 20°, they live longest; at 0°, their existence is shortest. The vitality of the nervous system seems to be thus directly influenced by temperature.

Of all the phenomena of animal life, those relative to the blood's state in asphyxia are, perhaps, the most interesting and curious, from the loss of consciousness, sensation, and voluntary motion attending its disoxygenated state. If, however, animals differ so materially under the influence of a deprivation of air, as to the duration of such existence, we may imagine a corresponding difference relative to their respirations modified by species, age, &c. Air, the *pabulum vitæ*, is not equally consumed by all, but in different proportions; at least, such is the presumption from the experiments upon animals of warm blood. The relative proportions of this difference are sought to be ascertained. Warm-blooded animals of equal size and age, at their liveliest period of age, were the objects of comparative inquiry. We must refer the reader to the table at the end of the work for the results. A marked difference is observable between the quantity of air consumed by the cold-blooded animals and that required for the support of the warm-blooded; and each has an organization appropriated to the individual distinctions. Thus the structure of the reptile and fish entails the lesser consumption of air, compared with that of the mammiferæ and birds. Fish consume least air, reptiles stand next, then the mammiferæ, and, lastly, birds consume most. The two last, however, very nearly approach each other; so do also the two first; and the distinction between the organization and the consumption of air is most strongly marked between the fish and reptiles on the one hand, and the mammiferæ and birds on the other, which, indeed, has caused their separation into two distinct groups, by the appellation of *cold* and *warm blooded animals*,—a distinction which clearly separates the whole of the vertebrated animals into two groups, bearing different physiological characters in their relations to animal heat and respiration.

The mere temperature of the blood in each group is insufficient for our knowledge of their distinctive characters. We

further find them characterised by a consumption of air in union with their heat, so as to unite these two functions, and thus render them dependent upon the same organs. Dr. Edwards has further shown, that from birth to maturity the production of heat goes on increasing with the consumption of air. And thus age (as well as the seasons) has been shown to be a modifier of animal heat; for, as the hot season advances, the consumption of air becomes diminished, and when the cold sets in, it increases; and this decrease and increase are accompanied by corresponding developements of heat.

In cases of fainting, of hysteric and asthmatic fits, the principle here laid down, as to the balance between the air consumed and heat, is instinctively acted upon by the most ignorant persons, who open all the doors and windows to admit cold air, and dash cold water in the patient's face. The addition or continuance of heat increases the affection. The application of cold produces instant relief. The state of asphyxia is relieved, the senses return, the pulse beats at the wrist, and the respiration goes on naturally. The *cooling* renders the air, unfit before, fitted for the purposes of life.

The effects of temperature upon the respiratory movements are indicated also in those constitutional changes which diminish the production of heat and the consumption of air. Organic affection of the heart or lungs may produce this change, which entails the necessity of a change of climate, or an alteration of temperature artificially, to restore the balance between the air and the animal heat.

A very elaborate and complete argument, and series of experiments, are devoted to the subject of *transpiration*, and the effect upon it of the influence derived from repose of the body and sleep, by the air's motion or stillness, and by the pressure of the atmosphere.

We have, however, pursued the interesting points touched upon so far as to render it impossible to enter at present upon this portion of the work. The importance of the subject demands a fuller investigation and report than we have now room for; and we must, therefore, defer it to another opportunity.

Experiments on Thought. By a Correspondent.

There is a very common prejudice respecting the rapidity of thought, which is imagined by many to be almost unlimited: and the opinion is very worthily illustrated by a reference to the oriental tale of a man's being bewitched into the belief that he had passed through a period of seven years duration, and full of the most striking vicissitudes; all in the time that he employed in dipping his head in a pail of water. Now there is no doubt that we often dream of a period of many years while we are only sleeping an hour; that is, we dream of an impression of a long continued existence, or perhaps of some detached fact scattered through such a period: but if any person will write down all that he can possibly recollect, of the separate imaginations that have passed through his mind in the dream, he will find that he will be able to read them over with ease in less than five minutes.

It is probable that there may be a considerable diversity in the rapidity of thought in different persons, as there is in that of muscular motions: but there is no reason to think the diversity greater. A healthy young man can run a mile in five minutes: a good pedestrian in four; but no man ever ran a mile in three minutes; and perhaps no horse in two. There is reason to think the rapidity of thought does not differ more materially than this in different individuals.

The rapidity of thought seems, however, more intimately connected with that of muscular motion than by analogy only: for they appear in some cases to be absolutely identical.

I have often been able to count ten in a second, in audible English words; not distinctly, indeed, but so as to assure myself that I do hear the ten words in their proper order; and to repeat the sounds for several consecutive seconds. If I say the words *to myself* only, that is, if I think them over, I cannot repeat them ten times in less than about nine seconds: I can never, for example, keep pace with my pulse, though it sometimes beats as slowly as seventy in a minute: nor can I, by any effort, think over the numbers from one to twenty in two seconds.

If I say to myself the first lines of Milton or Virgil, or

Homer, or any other lines that may be still more familiar to me, I cannot get through them much, if at all, more rapidly than I can pronounce them, even when I fix my undivided attention on them.

The rapidity of sensation is also intimately connected with that of memory and of muscular action. To cast the eye over a sentence, attending to every letter, is an operation which is capable of equal rapidity with the saying it over mentally: but it cannot be made much more rapid. It required four seconds to look over a sentence which occupied six in rapid reading.

The operations, which succeed each other with this limited rapidity, are not incompatible with a partial attention to other subjects: just as in running or walking, we may have our feelings very strongly interested by the sight of surrounding objects without interrupting the train of voluntary motions, which seems thus to be so linked together in a continued chain, as to become almost involuntary. And we may certainly be saying a thing over as rapidly as possible to ourselves, and may at the same time be seeing, and hearing, and even reasoning, so as to keep up what amounts very nearly, though not completely, to a continuity of attention to several distinct trains of ideas: in the same manner as the nerves of involuntary action are notoriously employed in several distinct trains of concatenated muscular motions and vascular actions, and as the ear of a musician is able to follow and retain a dozen different melodies in harmony with each other at the same time.

Dr. Darwin mentions an experiment which has a similar tendency to show the close connexion between thought and sensation. He says, that if we think intensely of a deep colour, for instance red, with the eyes closed, we shall see a tinge when we open them of the opposite colour, or green; just as if we had actually looked at a red colour instead of thinking of it. But I confess that I have never been able to satisfy myself completely of the success of the experiment.

These very hasty observations appear to me to be in great measure original; and the results of such experiments are certainly more calculated to illustrate the nature and powers of the human mind, than the fanciful hypothesis of the fashion-

able craniologists, with all their measurements of the heads of murderers, are likely to become.

ZMINIS.

London, 20 *Oct.* 1827.

POSTSCRIPT.—I find that some similar remarks have been made by the late Sir William Watson, in his Treatise on Time. He estimated, from some experiments made in company with his friend Herschel, the greatest possible velocity of sensation, such as to admit of about three hundred distinct impressions on the eye or the ear in a second. "It is true," he observes, "that whoever attends to what passes in his imagination on particular occasions, will be struck at the apparent rapidity with which ideas appear to flow at times, and will be apt to suspect them far to exceed sensation in that respect. But it is probable that we are ourselves deceived in such cases." P. 38. But there are no direct experiments to prove this opinion. On the other hand, a sound may be continuous, and yet consist of only about twenty vibrations, or still fewer, in a second.

HIEROGLYPHICAL *Fragments, illustrative of Inscriptions preserved in the* BRITISH MUSEUM, *with some remarks on* MR. CHAMPOLLION'S *opinions. In a Letter to the Cavaliero* SAN QUINTINO. By a Correspondent.

My dear Sir,

You will be glad to hear that I have made some little progress in the study of the Enchorial inscriptions which I had lately the pleasure of showing you; my steps have, as usual, been guided by no *system* whatever: they have been wholly *empirical*, and though very slow, I trust they are so much the more sure; and I hope they will at least serve as an excuse for my reminding you of the expectations you kindly allowed me to entertain, that you would send me copies of any thing of the kind that you might find among the objects entrusted to your care at Turin. What I have lately done has only been to ascertain the dates of many of the tablets sent by Mr. Salt

from Sacchara, all of them about the time of the last Cleopatra: to identify the Enchorial name of Ptolemy DIONYSUS, and to make out a passage relating to a donation of MUCH GOLD AND SILVER AND GEMS TO THE SANCTUARY OF THE GREAT GOD AT MEMPHIS. The different forms of the characters employed by the writers, in the same words, constitute also a valuable addition to the means of deciphering any new inscriptions of a similar nature, and I have already incorporated many of them with my little Enchorial Dictionary.

The 48th and 49th plates of the Hieroglyphics, already published, contain two tablets, apparently funerary, but without any dates of the reigns: the ages of the persons seem to be expressed in the hieroglyphical lines. In the 49th we find the name Berenice twice in the Enchorial letters, and once in hieroglyphics; followed here by Arsinoe, possibly as her mother.

This tablet, coarse as it is, abundantly shows that Horapollo and Champollion are both correct, independently, as it seems, of each other, in considering the rings, or cartouches, as chiefly confined to the names of royal personages; and that I inferred the contrary somewhat too hastily, from observing that the *imitations* of those rings were attached in the Enchorial inscription of Rosetta, to several names not royal, and from having found such rings in other hieroglyphical inscriptions, without the usual epithets of kings. I had, indeed, remarked, that a "mysterious" name was *sometimes* observable in the manuscripts without a ring, and I had pointed out the same group as *a* name in Lord Mountnorris's manuscript, which Mr. Champollion considers as the true name: but I am perfectly ready to admit that Mr. Champollion has materially *improved* on this hint, as he has on many others.

The same line of hieroglyphics, however, contributes to add to my reluctance in admitting Mr. Champollion's reading of P.T.H; a group which I considered as very probably representing these letters long before the date of his publications; though I had only fully identified the two first characters; it seems to me to agree better with PETEH than with PHTAH; and I am inclined to think it was the beginning of the names Petosiris, Peteharpocrates, and other similiar words,

as it is here annexed to the names of two or three other deities. But I am by no means confident on the subject; and beg only to be allowed a few years more to collect further evidence, without being accused of *resisting* conviction.

I must also claim a similar indulgence for my opinion respecting the bird and the disk which is so constantly found between two names, that I could not avoid supposing it to mean simply *son;* I confess that the arguments which Mr Champollion has drawn from the application of this character to some of the Roman names, as well as those which Mr Salt has deduced from the inscriptions which he has published, are at least sufficient to silence me; I had, indeed, long before observed that the first name of one pair of rings is scarcely ever found as the second of another, though I fancied the Minervean obelisk might afford an exception. On the other hand, I cannot explain, upon Mr. Champollion's theory, the order of the names in the tablet of Abydus which might be supposed to have been purposely intended to perpetuate this discussion.

It is admitted that this tablet contains the names of a chronological series of kings, each characterized by one ring, containing what I have always considered as the true names of the persons in question. It is easy to grant to him that they are the praenomens only, as is common in all modern chronology. But how comes it that there is one exception to this, and that the reigning monarch is characterized by his second name only, where he first occurs, and where we should expect to find his father? This is precisely what would have been required if the document had been forged to support my opinion; though I should certainly have been very ungrateful for an argument, which is more calculated to increase the difficulty than to remove it.

An objection of a similar nature may be deduced from the tablet found between the legs of the sphinx, and copied by Mr. Salt, H. 80. The "Mesphrès son of Thuthmosis" of the Article Egypt is represented naturally enough as doing homage to his deified father, under the form of an Androsphinx; had he been doing homage to himself, the names would scarcely have been so divided. They also occur repeatedly afterwards in the inscription, but never together.

The tablet represented in Plate 51, is remarkable for the confirmation which its date affords of the accuracy of our chronology of the Ptolemies. It has no *pure* hieroglyphics. It begins immediately with "*The year* 19, *otherwise* 4, *of Cleopatra* [*Neotera*], *and Ptolemy surnamed Caesar*: that is, the year 34 B. C.; and the same date is repeated in a form somewhat more distinct, four times in the 10th, 11th, 12th, and 15th lines. In the last it is followed by *the Queen gave to the Priests and High Priests* .. then *Ptolemy* [*Auletes?*] .. *Queen Cleopatra and King Ptolemy surnamed Caesar.*

It has before been observed, that the word *surnamed*, as it occurs in these tablets, and in Mr. Grey's manuscripts, comprehends the characters which answer to the NEO of Mr. Champollion's NEOCAESARI. The beginning of the group occurs elsewhere in the sense of *called*, and can scarcely be read "ETO," whether we consider the sacred or the enchorial characters; nor do we find any thing nearer to this in Coptic than ETE, meaning "*that is*," while the characters are more like TENE. Such are the uncertainties which continually beset us in the application of the best established alphabetical characters even to words of which we know the sounds: to investigate the unknown by them is at present almost hopeless.

There are two tablets, from the caverns at Sacchara, about to appear in Plates 70 to 74 of the Hieroglyphics, which Mr. Salt sent over with particular interest, as being likely to contain some useful materials for the comparison of the different kinds of characters with each other. In this point of view, however, his well-directed zeal has failed of its object: for the sacred characters relate almost entirely to the gods and priests of the temple, while the enchorial inscriptions below them contain dates and records of the successive donations made to those temples. And this seems to be equally true of the generality of double inscriptions, which are scarcely ever identical in this sense, although they may greatly tend to illustrate each other.

The first in order of these tablets (H 70, 71, 74 A) was marked number 50 by Mr. Salt; it has seven stars at the edge of the wings overshadowing the figures. It is first dated very distinctly *In the year* 6 *of Cleopatra*; which ought to have

been 6 *otherwise* 2; but the second date was perhaps omitted after an interval of more than 20 years, which must have elapsed at the time of putting up the tablet, as the subsequent dates demonstrate. The queen seems to be styled *Isis*, but the name of the "younger goddess," which is found on her medals, does not appear in these inscriptions. In the 4th line the word *Memphis* occurs, though less distinctly than elsewhere. It seems to be formed of characters meaning *Temple*, and *Good*, and might naturally be read PHE-NUF; which agrees sufficiently well with the NOPH of Jeremiah, translated Memphis by the Septuagint, as well as with the Coptic PANUF, said to have been *Momemphis*. It is possible that Phthah may have been meant by the Good god, NUF; but there is here no character at all resembling the Enchorial name of Phthah, which approaches to that of a figure of 4.

We next find a notice of the change of dynasty (Line 5) . . year 7: the *Gods Phre and "Horus" and 'Phthah? gave the victory to* AUTOCRATOR CAESARIS *the Munificent.* The number 7 is indistinct; if correct it must belong to the later of the double dates of Cleopatra's reign, which terminated in the 22nd or 7th, the year of the Battle of Actium, in which the *victory* was obtained by the *Emperor* Augustus *Caesar*: Then follows a date of the *year* 6, probably of Caesar: and the seven stars of the wings may possibly relate to the erection of the tablet in the subsequent year. We have also a donation of *gold and silver gems.*

The second tablet (H 72, 73, 74 B) has first the date of the *year* 19 *of King Ptolemy* [Auletes], the *Defender of the sacred rites* (L. 3) . . *The year* 4 *of Cleopatra* '*Neotera?* (4) . . *many years* . . (5) The year 7? *the gods* '*Phre and Horus and Phthah? gave the victory to the Emperor Caesar,* 'and Phthah and Horus who loved him gave the dominion of all men to? *Caesar.* (6) . . *gold and gems and silver in abundance, gave them to the sanctuary of the great god in the temple of Memphis* . . The year 7, of Caesar: 'Mechir 18? gave to the sanctuary of the great god in . . (8) . . *gold and gems and silver* . . (9) *Memphis.*

We have here no subsequent year 19 to which the stars of the margin can refer: and it seems therefore most natural to

suppose that they belong to the earliest date, with which the tablet commences: and perhaps the seven stars of the former may have been marked by mistake for six. The interpretation of the marginal stars will be easily brought to the test of future observations.

Plates 75 and 76 contain portions of a large tablet from Sacchara, very fairly written on chalk. of which the upper part is broken off, leaving only a few traces of a hieroglyphic inscription, which seems to have contained a date at the end, perhaps the 12th of Mechir.

(1) [In the . . year of Queen Cleopatra] and *Ptolemy surnamed Caesaris*, the divine king . . living for ever. (7) . . *The year* 9, Athyr or Mechir 9, *of the great King Ptolemy* the god ' Brother of Apis ? DIONYSUS ' the awful ? living for ever . . (19) . . *the great King* Ptolemy the god ' Brother of Horus ? DIONYSUS . . . mighty as the sun ? . . . (20) . . . living for ever . . (21) In the year 7 Mechir the 14 . . *The Queen Soter and King Ptolemy surnamed Caesaris* living for ever . . gave . . (25) . . children, for ever. 28 . . ' Written and engraved by ? . . .

In the 79th plate there are four enchorial lines very distinctly written, and beginning with a date, which must be either 24 or 28, and most probably the latter, as there are 28 stars in the margin : perhaps the 11th of the month, in the reign of *Ptolemy the son of Ptolemy, may he live for ever.* The rest is not intelligible.

In this manner, my dear Sir, I have been creeping, while others have been flying, though perhaps a little too near the sun. Possibly my friend Champollion, and *your friend* Seyffarth, would be able to decipher much more of these inscriptions ; and it is probable that their versions might differ in almost every particular. In this case it is unnecessary for me to say which of the two explanations I should be inclined to prefer : for it is impossible to deny to Champollion the merit of great industry, and deep, as well as extensive research. I object only to his precipitation, and his love of system, which, I think, cause him to be led away by his own ingenuity, through a series of conclusions unsupported by sufficient evidence.

As an instance of a hasty and u[illegible]tion, I shall mention his explanation of the group of characters, which he considers (Système, p. 82) as "forming the third person plural of the future in all the verbs of the last nine lines of the hieroglyphical text of Rosetta, expressing the different dispositions of the decree, and answering to Greek verbs which are always in the infinitive," and which he naturally enough reads SNE.

There is nothing absolutely incorrect in this statement, but the reader naturally infers from it that the group in question occurs either exclusively or principally in these nine lines. The fact is, however, that in the first five lines, or rather half lines, the group is found ten times, and in the remaining nine, only eighteen, that is, about half as frequently, in proportion to the actual length of the lines: nor can I find any where a context that favours Mr. Champollion's interpretation; though I have lately observed that an Enchorial group, resembling 'O, is found almost uniformly to answer to the Greek infinitive: being read perhaps MNR or MARE: but I cannot make these characters agree either with the hieroglyphics in question, or with the sounds SNE, which Mr. Champollion attributes to them.

So little is Mr. Champollion in the habit of distinguishing *proofs from assertions* in his own case, that it is the less surprising that he should sometimes confound them with respect to others. He says, for example, with respect to the nature of the Hieratic characters, which he explained to the Academy of Belles Lettres in 1821, "*je me suis convaincu depuis que M. le Dr. Young avait publié avant moi ce même résultat, et de plus, que nous avions été* PREVENUS *de quelques années, l'un et l'autre, quant au* principe de cette découverte et sa définition, par M. Tychsen de Goettingue." (p. 20.) Professor Tychsen had *asserted* this agreement as a probable opinion: it was amply *demonstrated* in 1816; *five years* afterwards Mr. Champollion thinks he has a right to consider himself as a new inventor of the doctrine, because he chose to neglect what was done in a neighbouring country, and to undervalue the actual *proof*, in which he had been anticipated, by classing it with a bare *assertion* to be found in a German publication.

Prec[illegible]it he remarks, in the next page, that Bart[illegible] *pointed* out the rings as containing pro[illegible] they had, indeed, *said* that they might be proper names, or moral sentences, *or* something else; but the only question was, if it was worth questioning at all, to whom belonged the priority of the *demonstration* that they actually were proper names: which, before the publication of the Archaeologia for 1814, was no where to be found. This publication was the first great step after the discovery of the pillar of Rosetta: the second was the identification of the different kinds of characters, in 1816, by means of the Déscription de l'Egypte: the third, the application of that identification to the names of Ptolemy and Berenice: the fourth, perhaps, was Mr. Bankes's discovery in Egypt, of the name of Cleopatra, which he sent to Paris: and on these grounds is certainly *founded* ALL that is at present known of Egyptian literature, for a very considerable proportion of which we are unquestionably indebted to Mr. Champollion.

The French translator of Mr. Browne's ingenious articles which appeared in the Edinburgh Review, has certainly gone a good deal out of his way to find matter of accusation against Mr. Champollion. He quotes the text of a memoir published in 1821, and afterwards *suppressed*, in order to show that Mr. Champollion then continued to believe that the hieroglyphics were signs of things and not of sounds; and that he disagreed with those learned persons who had considered the hieratic writing as alphabetical. The date of this suppressed paper is indeed of some consequence, as determining the period at which Mr. Champollion made his rediscovery of what Dr. Young had published in 1816; that is, the fact of the essential identity of the two systems of writing. But the translator might have found in the beginning of the letter to Mr. Dacier, dated in 1822, the same opinion respecting these systems of writing; that is, the *hieratic* and *demotic*, which, he says, are not alphabetic, but "*ideographic*, like the hieroplyphics themselves," expressing ideas and not sounds; and he adds, that *he* (!) has deduced from the demotic inscription of Rosetta a series of characters which have a "*syllabico-alpha-*

betic value," by which foreign pro[redacted]ed. (p. 2.)

Nothing can possibly agree better than this with the opinions which Dr. Young had long before published; and which he has since confirmed in his octavo volume; and if Mr. Champollion's ideas upon this subject have sometimes appeared to fluctuate, it has probably been more from a love of system, and a wish to establish originality, than from any new discoveries that he can have made respecting these two modes of writing in particular.

What precise forms of characters may be supposed to answer to the sense in which Mr. Champollion employs the word demotic, cannot very easily be ascertained. It is remarkable that his "SNE" is a group very commonly found in the manuscripts of the Déscription de l'Egypte, which Mr. Champollion might possibly call demotic; while it cannot be identified in the Enchorial Inscription of Rosetta. This is an instance of the difficulty of finding appropriate terms where we have not exact definitions. The difficulty is not avoided by the use of the word Enchorial, except that it may with perfect safety be applied to such inscriptions as are capable of having any of their words identified with the inscription so called on the pillar itself.

The verification of the chronology of Manetho must naturally be a work of time, even after the complete identification of the names of the kings, which cannot *yet* be admitted to be satisfactory. There is one discordance that it may be right slightly to point out, as it is presented by Plate 43 of the Hieroglyphics: we there find the 29th year of the Sesenchosis of Manetho; and Manetho allots but 21 years to this king, who was the first of his dynasty, and could not, therefore, like Philadelphus, have continued any era from an earlier period.

It is easy to observe, in comparing Mr. Cailliaud's copy of the Tablet of Abydus, as published by Mr. Champollion, with those of our countrymen, Mr. Bankes and Mr. Wilkinson, contained in the 47th plate of the Hieroglyphics, or with the manuscript copy of Mr. Burton, how much more hastily the French traveller had executed his task than *any one* of the three Englishmen.

Another of Mr. Wilkinson's very valuable inscriptions, from a temple at Kous, must be allowed to give evidence much more favourable to Mr. Champollion, as far as it regards the signification of the *plough*, which seems to enter into the composition of *Philometor*, as applied to Cleopatra and "Ptolemy Alexander," who are called Philometores Soteres, both here and in Anastasy's Greek Manuscript. The name of Alexander had never occurred to the author of the article EGYPT, but he had evidently a foresight in what way it would make its appearance when he observed, N. 55, "it will appear hereafter, that a knowledge of the enchorial forms may possibly contribute very materially, at some future time, to assist us in determining it:" and he immediately proceeds to the subject of PHONETIC HIEROGLYPHICS.

The plough seems to be exchanged on the Minervean obelisk for the dentated quadrant and chain, which may hence have been synonymous with the dentated parallelogram or comb: both perhaps having represented instruments which bore the same name, and served the same purposes, though of different forms: they may, for instance, have been rakes or harrows, and may hence have borne some analogy to the plough or hoe. Whether they had names beginning with M, may still be questionable.

Mr. Champollion has endeavoured to explain the absence of the names of our queens from the tablet of Abydus, by saying that it must be considered as a tablet "purely *genealogical*." First Letter to the D. de B. p. 89. A reader is naturally disposed to acquiesce in this explanation, since Mr. Champollion, who has carefully examined it, asserts it on his own credit; especially as the assertion appears to be supported by a long and minute discussion. Unhappily, however, it is only necessary to compare his brother's chronology in P. 107, with his own Plates II. and III. fig. 5, from which it appears that Amenses, who *reigned* more than 20 years, was the mother of Thuthmosis the second, whose name is in the tablet, while his mother's is *omitted*. It is true that, with his usual ingenuity, Mr. Champollion seems afterwards to change his ground in the same page: for he says, that one only of two brothers or sisters was inserted, in order to keep the number of the

generations unaltered: and he might have added that Amenses was the sister of Amenophis, whom she succeeded. If he had stated this clearly, the reader might have judged for himself, whether such a coincidence was or was not sufficient to support the chronology of Manetho; which was, however, by no means in want of *such* support: in the article Egypt, for example, Manetho's chronology of this dynasty is fully adopted: and the same 'cartouche' is read *Thuthmosis*, which Mr. Champollion, after all his parade, still admits to be Thuthmosis: nor is there a difference of half a century in the dates assigned to his reign by various chronologists. It was established in the article Egypt, that the name contained that of Thoth, the Egyptian Hermes, and for this reason it was considered as better established than any other of the names of the Pharaohs. Mr. Champollion had never discovered this for many years afterwards: and yet we have been told by an Englishman in the last Quarterly Journal, that to Mr. Champollion the *greater part* of the *discoveries* made by the interpretation of hieroglyphics are owing!

Believe me, dear Sir, very sincerely, yours,

* * * *

London, 24 *Nov.* 1827.

On the Naturalization of Fish. By J. Mac Culloch, M.D., F.R.S., &c.

Dear Sir,

As I promised you that I would communicate to you, from time to time, any new remarks or facts which might occur on the subject of naturalizing sea-fish in fresh water, I am pleased to have an opportunity of noticing a few circumstances which may serve to keep alive in the public mind a subject, from which I cannot yet help foreboding useful results, in spite of the neglect and opposition which it has experienced from every person, I believe I may safely say, to whom it has been proposed, except Mr. Arnold. I am perfectly safe in saying, that, with this sole exception, every individual to whom the facts have been described, and the experiment proposed, have replied by doubts or cavils, or objections of some kind; many, by

positive disbelief of the very facts; while the far greater number have been persons, whose entire ignorance of every requisite point of physiology, natural history, and chemistry, must, of course, have rendered their objections sufficiently unworthy of notice, though not sufficient to restrain the confidence with which they have been urged. The satirical writers of the day view this as the character of the age: the more obvious aspect which this disposition presents, is the feeling, as if he who attempted, by suggesting an improvement, to render a service, was meditating an injury, and was an enemy, to be opposed at all hazards. I must permit you to settle metaphysical and moral questions so profound as to exceed my own ingenuity.

But I cannot avoid regretting that Mr. Arnold is not the rich and idle proprietor of some of the tens of thousands of acres of fresh water, whether Scotch or English, in which a 'sea-fish cannot possibly live,' or 'would certainly not be eatable:' and, in addition, that, instead of a not very opulent and very busied 'notary public,' he was not in possession of some five thousand of these acres, with as much money, and as much leisure. And I feel bound to add to this apology for what he *has not* yet done, that the expense of such a course of experiments is considerable; at least in this comparison. A superintendent would be necessary; and for the purpose of taking and transporting the fish, still more of drawing nets periodically and frequently, to ascertain the progress of the transplanted fish, there must be expensive assistance, for which, as yet, there can be no returns; while that, in addition to irregularities and rocks in the pond itself, impeding the accurate drawing and examination, must also be the apology for the imperfection of the present additional report as to the success for certain fishes. It is plain that, though ten or a hundred turbots were present in a pond of four or five acres, the fact is not one that can easily be ascertained. Let those who have money, leisure, and water, and nothing else towards the investigation of this object, restrain, at least, *their* incredulity and opposition; as may also they, very safely, who never saw a fish, except on the stall of a London fishmonger.

With respect now to some facts: it had been said that the water was salt, because this pond was situated at a sea embank-

ment. I stated before, that it admitted the sea, by leakage, in summer, when there was little comparative supply of fresh water, and was therefore brackish, or saline. I have since ascertained the exact proportion of salt in the water, at those times when the fresh water is least. In the driest and hottest part of one summer, the proportion of salt in it, as compared to the sea without, was as 40 to 150. In another, peculiarly dry, 1827, it was one half; and the water, having then been at the lowest, it cannot ever be computed to exceed this. Moreover, this period of saltness cannot easily, even in such a summer, occupy more than the months of June, July, August, and September: or, more strictly speaking, it is probable, scarcely one half of that time in general, in so rainy a climate; a climate equalling Penzance in the quantity of rain.

In winter, that is, during five or six months, or less, if any objector pleases, it is fresh. That cattle drink it freely, is not an exact chemical proof; but I must admit, that I have not analyzed the water at that period, holding the objection in great contempt. It may be sufficient to say, that it then occupies a space of about sixteen acres, or increases to this magnitude from four and a half acres; so that it cannot, at least, be very salt, while the fish, and the mullet in particular, are found in the remotest ditches, among the meadows. But, in defect of an analysis, which I have not had the means of making, there is a valid reason why the water should be fresh when the size of the pond is much extended. The presence of sea-water in it, is, in all cases, the consequence of a depression of the water within the sea-wall, which allows of leakage or infiltration at the upper part, so as, in high tides, to equalize, as far can be done in the short period of high water, the levels within and without. This, it is plain, must cease whenever the water within is higher than the sea without; and hence it is that there can be no access for the salt water in the winter or rainy months.

Enough of the mere fact: the objections derived from which ought not to demand an answer among physiologists; while to those [illegible] physiological points in utter ignorance of all that [illegible] to physiology, it is probable that all answer is fruitle[illegible] as stated before—the question is simply twofold;

respiration and food. If fish can breathe indifferently salt water or fresh, for one week or one month, and if, in their new element, translated from salt to fresh, they thrive or grow, fatten and breed, the trial of three weeks or three months is a sufficient proof that they will neither sicken nor die of fresh water. If they can find food, it is indifferent whether the medium is fresh or salt. It is the misfortune of the age to understand every thing without knowing its principles; just as every man is now a physician. A few, more profound, who chance to know that salmon divide their time between fresh and salt water, possess other reasons, and find other objections; which they must be permitted to explain for themselves. I ought not, while on this particular subject, to omit one fact, which has come to my knowledge since the former papers were written, on the voluntary emigration of a fish, supposed to be peculiarly delicate and peculiarly attached to the sea, into fresh water. This is, that, in Virginia, the herring ascends the rivers, even up into the most minute communicating branches, and as far as it can reach; while a somewhat recent traveller describes them as being so abundant, that it is impossible to cross the fords on horseback during the season of their migration, without destroying them by the horse's feet. To proceed to the *historical* condition of this pond.

I have already stated the difficulties arising from want of leisure and wealth in the proprietor, added to non-residence I should say, whence chiefly has arisen the difficulty of tracing the results. Let those try for themselves, who consider that all this might have been ascertained in a twelvemonth, and with the same means. Since the communications I formerly made, the Pilchard has been introduced. It swam away briskly, therefore it would not die of the fresh water; but it has not been retaken. The retaking of individual fish, to ascertain their presence, is a fundamental difficulty, as I before pointed out.

The Brill has also been introduced since my former list. It has been retaken; and, within one year, had grown to double the original size.

The Turbot. Fifty or sixty were introduced, averaging about eight inches in length. Some were retaken in a year, for the purpose of examination merely, like the former and most others;

they also had grown to double the size. There is no prospect of dying in these cases, it is abundantly plain; that they will breed is probable, but there has been no time, nor would the young have been taken. What is to prevent healthy fishes from breeding? The young, indeed, may be eaten; if so, it is for want of room, or want of a proper balance in this mixed population. No one knows any thing, either of the ordinary growth, propagation, or destruction of fishes; and how then can any one decide on what is regular or extraordinary?

The Wrasse has been retaken after a considerable period; therefore it is not dead.

The Basse has propagated; and so has the Brill.

The Red Mullet has been introduced, and is living.

The Whiting was introduced, and taken in good health many weeks after, but not since.

The Grey Loach is thought to have bred considerably.

The Atherine continues to breed.

I formerly mentioned that the flavour of the several fishes was improved: this is now more positively asserted, in addition, of the Basse, the Plaice, and the Red Loach. Others were mentioned in former communications.

Loss of property, or flavour, has been made a speculative objection by the unvarying objectors. General experience has shown, that in all fishes, as far as known, the access to fresh water, or fresh water food, improved the flavour; in many, in oysters, muscles, cockles, shrimps, it is vulgarly notorious; as in mere sea water they are worthless.

There is a popular objection, on this head, made by the country gentlemen, which I must answer; to those who *think* about what they know, it would have been superfluous. The salmon is good when it comes from the sea, and bad when it is returning. Doubtless, it is; while the reason ought to be plain, even to an angler. It is in full health in the first case: in the latter, it has spawned; and, at that period, every fish is proportionally as bad as the salmon; many are a great deal worse. The fault is not in the water, nor probably in the food; it is in the spawning, and with any food the same effect takes place, in all fishes, everywhere.

I suggested in former communications, that an essential point

to ascertain, in any view of economy, or management, would be the proper balance of species; to discover what kinds would so live together that all the species might find food; might breed, each to its useful limits, so as to be serviceable to ourselves, the keepers of the flocks, and without hazard of the extermination of any kind. I may illustrate what is here meant, by a simple fact, in the ordinary economy of fresh water fishes in confinement. Pike and perch can live together, because the natural defences of the perch prevent the pike from exterminating the race, voracious as the enemy is. If trout and pike were confined in a narrow water, the trout would be destroyed.

Or otherwise, it must be our object to ascertain, in an economical view, how to feed, by means of species that we do not desire to eat, those which we do cultivate for our own uses. This is a difficult question, which can only be overcome by time and experience; by knowledge; by knowledge, when we are in a state of entire ignorance; ignorance of every thing that relates to fishes, as great as if they were the inhabitants of another planet. This was one great source of difficulties with us in this case; and I, myself, must plead guilty, I fear, to a general recommendation of introducing every fish as a mere matter of trial; the result of which has been mischievous. The basse appears to have been the great enemy; to have eaten up the greater number of many species, and given no return. It has proved the pike of this pond. This could not have been foreseen; it is a caution for future speculators. Others will be discovered in the course of trial. It appears also that the common crab has proved destructive, probably by eating the spawn of larger fishes. From some enemy or other, the eels, which at first abounded to an incredible degree, have most materially diminished, and so have the shrimps. The latter, at least, appear to have been destroyed by the basse. Time and trial will teach us what to do in this case; in the infancy of ignorance, man might have supposed that he could keep wolves and sheep in one field, and have constructed a pen for foxes and fowls, rabbits and weasels. We must not accuse nature of our own ignorance.

The question is here a difficult one; but a little more study

of the general habits of fishes, merely as we know them already, and even of their anatomy, will go far to lay the foundation of useful rules on this head, even without a hazardous trial, which may ultimately not become in our power to remedy, as I much fear may prove the case with respect to these unlucky basse. Not to enter on this further than as it may serve for a general illustration of what is here meant, the anatomy of the mullet proves that it lives on worms; on the lumbricus marinus, and others; and so do its habits. So also may the very food of others, as found in the stomach, serve to indicate their natural or ordinary food. Reversely, the anatomy of a cod's jaws, and its stomach also, prove it to be omnivorous, omnivoracious. Or, further, the anatomical character of the diodon proves that it eats shell-fish; as we are equally able to limit the range of food in the flat-fish which have no air-bladders, and cannot quit the ground.

But in this brief communication, I must not enter further into this subject than is necessary for mere illustration. I may take some further opportunity to point out the probabilities, as to mutual food and protection, in any artificial cultivation of this nature, as they might be derived from studying the little that we do know about the structure and habits of fishes. All that I need add here, is, that I have suggested the introduction of limpets, periwinkles, and cockles; as affording food without furnishing enemies: a matter which had been overlooked. To exterminate the enemies which have been unwarily introduced, will not prove so easy a task; unless, at least, we could find their natural enemies; find the great secret by which alone, in all cases, man can make war on those whom neither his artillery, his physic, nor his politics can reach.

The transportation of fishes has been objected to as difficult. I had occasion to make some remarks on this formerly, and on the vitality of some kinds. The difficulty is not so great as has been imagined. The fact generally is, that fishermen, even down to the very sentimentalists who worship the gentle Izaak, and who are sometimes scarcely possessed of the wit of a fish, treat them as they would a stone; as if they had not lives, and wills, and opinions, and were not part of the same

creation as ourselves; as if that creation, which outnumbers ourselves by millions of millions almost beyond algebra to express, was not, like ourselves, under His care. They are easily killed by violence; they kill themselves by over-exertion, from anatomical peculiarities; as every trout-fisher knows; that is to say, the fact, not the cause. Let them be treated with gentleness when taken, as if they could feel; and they will not die in being removed into a cask of water. The flat fish are all peculiarly tenacious of life, so are all those of firm muscles generally: the vitality of the carp and of the minnow also is notorious; and so it is as to many other kinds. All these can be removed and carried far, even in straw; but in truth, he who chooses to make his experiments like a philosopher, and who desires to succeed, will not fail.

Yet let me point out what I have suggested to Mr. Arnold, among other things: to him, whose merit as an ardent experimenter, always ready to adopt a reasonable suggestion, and never seeking for an objection, ought to stamp his character as a genuine follower of the true philosophy; the exception, in this particular case, to every one else. This is, to adopt the Chinese method of transporting the spawn of fishes; as affording a far greater facility to the introduction of species. I presume that the general fact must be known to your readers; though I believe that I ought to doubt: because I quoted the same practice from Columella formerly, as in use among the most ancient Romans, among the common farmers.

This substance is perpetually brought up by the trawl net, very injuriously, as it relates to fisheries; and in many cases, the fishermen contrive to guess tolerably well to what fish it belongs. That it may be transported to any distance, the familiar practice of China proves: since it is there a common article of sale in the markets; while there also, I may incidentally remark, the cultivation of fish for sale, their transportation to market, and their replacement in the ponds, if unsold, is as much matter of ordinary farming as the management of a poultry-yard; while the pond is often the most profitable part of the farm. They also, who do not already chance to know it, may be informed, that this species of poultry-yard, or fish-pond, is as easily and regularly

stocked in this manner, and managed, as any other portion of the farm: since it is even destroyed, or suffered to become dry occasionally, and again renewed in the wet season, by the means of purchased spawn, or stock; just as a sheep farmer buys lambs to stock his mountains. If England is too wise to learn of Rome or China, or of France and Germany, or even of the experiments on which I have dwelt so much and so often, it must be a pleasing reflection that it is already so amply informed as to have passed the bounds of all possible improvement and all possible wishes. But that I may terminate this particular suggestion, I will only further point out, that lobsters, and the crab tribe generally, might very easily be transported in this manner, and that, in them, it is easily known when the *ovum* has been impregnated, by means of a black spot with which it is then marked.

If I ought to apologize already for the length of this communication, I shall conclude it by saying, that whatever may be judged of the general philosophy of this subject, there is not and never has been any thing to prevent the cultivation of fish, in ponds of salt-water at least, or the preservation of them in any water in which they will live for a sufficient length of time, so as to render that a depôt for the purposes of a fish store, calculated for the steady supply of a market, in the manner which I formerly described and proposed. If, after so many years as this proposal has been made, London has not seen either the facility, or the utility, it will discover them at some future day; just as it discovered, ten years after there had been twenty-six steam-boats on the Clyde, that a steam-boat might possibly be of use on the Thames; just as it opposed gas-lights, and just as it has adopted gas-lights.

Nugæ Chirurgicæ; or a Biographical Miscellany, illustrative of a Collection of Professional Portraits. By W Wadd, Esq., F.L.S. &c. 8vo. pp. 276. London, 1824 Longman and Co., and Callow and Wilson.

Nugæ Canoræ; or Epitaphian Mementos (in Stone-cutter's Verse) of the Medici Family of Modern Times. By Unus Quorum. London, 1827. Callow and Wilson.

Mems., Maxims, and Memoirs. By W. Wadd, Esq., F.L.S., Surgeon-Extraordinary to the King, &c. 8vo. pp. 303. London, 1827. Callow and Wilson.

We have placed these three Works together, because they are so closely allied as to form a whole, and also because *Nugæ Canoræ* and *Nugæ Chirurgicæ* bear internal evidence of being written by the same pen; and when we say further, that they are characterized by good feeling and good humour, we are sure we are not far off in our *guess* about who is 'Unus Quorum.' These volumes come within the scope of our Journal, as comprising an outline of the history of medical science, sketched in a vein of pleasantry that makes it no less agreeable to the general than to the professional reader, and we have derived both amusement and information from its perusal. Like the 'Gold Headed Cane,' it helps us to much curious modern biographical anecdote, with the addition of varied entertainment for the medical antiquary. While, however, we recommend these 'Mems.,' and commend the literary loungings of contemporary practitioners, we cannot but regret the neglected volumes of Aikin and Walker, and lament that the lack of feeling for the annals and literature of their profession, should be less active in the medical public of this country, than with our professional brethren on the Continent.

'*Nugæ Chirurgicæ*' is a *Catalogue Raisonnée* of a scarce collection of Medical Portraits. We believe only 250 copies were printed; from which circumstance, and its recording the congregation of the greatest assemblage of medical men ever met together, it is probable that it may some day become a medical rarity. The author's original intention appears merely to have been to describe the portrait, with some characteristic trait; but an after-thought seems to have occurred, and in the "Memorabilia," the "*Medici Family*" are, as it were, retouched and varnished, so as to become

very agreeable pictures. We shall now present our readers with a few specimens of this gallery, taken at random.

EURICUS CORDUS.

"Cordus, who was accustomed to receive his fees only at the termination of his patient's disease, describes, in a facetious epigram, the practitioner at three different times, in three different characters.

> Three faces wears the doctor; when first sought
> An angel's—and a god's the cure half wrought:
> But when, that cure complete, he seeks his fee,
> The devil looks then less terrible than he.

"The epigram of Cordus is illustrated by the following conversation, which passed between Bovart and a French marquis, whom he had attended during a long and severe indisposition. As he entered the chamber on a certain occasion, he was thus addressed by his patient: 'Good day to you, Mr. Bouvart; I feel quite in spirits, and think my fever has left me.' 'I am sure of it,' replied the doctor; 'the very first expression you used convinces me of it.' 'Pray explain yourself.' 'Nothing more easy: in the first days of your illness, when your life was in danger, I was your *dearest friend;* as you began to get better, I was your *good Bouvart;* and now I am Mr. Bouvart: depend upon it you are quite recovered.' Bouvart's observation was grounded on a knowledge of human nature: every day's experience shows, that '*accipe dum dolet*' should be the medical man's motto.

JOHN CASE.

"In one of the profound pieces of astrological bombast written by this singular genius, he gives an account of the creation of Adam: '*Thus* Adam was created in that pleasant place *Paradise*, about the year before Christ 4002, viz. on April 24, at twelve o'clock, or midnight.' His name was latinized to *Caseus*, which was occasionally interpreted Dr. *Cheese*. Granger says the following anecdote of Case was communicated to him by the Rev. Mr. Gosling, in these terms: 'Dr. Maundy, formerly of Canterbury, told me, that in his travels abroad, some eminent physician, who had been in England, gave him a token to spend at his return with Dr. Radcliffe and Dr. Case. They fixed on an evening, and were very merry, when Radcliffe thus began a health: 'Here, brother Case, to all the fools your patients.' 'I thank you, good brother,' replied Case; 'let me have all the fools, and you are heartily welcome to the rest of the practice.'

THOMAS DAWSON.

"The following anecdote is related of him: After he became M.D. he attended his neighbour Miss Corbett, of Hackney, who

was ind[illegible] found her one day sitting solitary, piously and pensively musing upon the Bible, when, *by some strange accident*, his eyes were directed to the passage where Nathan says to David, 'Thou art the Man.' The doctor profited by the kind hint; and, after a proper time allowed for drawing up articles of capitulation, the lady, on 29th May, 1758, surrendered herself up to all his prescriptions, and the doctor very speedily performed a perfect cure.

PHILIP HECQUET.

" 'C'est une erreur de penser que le sang soit nécessaire à la conservation de la vie ; on ne peut trop saigner un malade ;' are the words put into the mouth of our doctor, in the character of Sangrado by the facetious Le Sage. Hecquet, both in theory and practice, carried the *anti-phlogistic* system to a greater extent than any other man, and defended the '*boisson*' and the *bleeding*, saying, 'J'ai pour garants de mon sentiment, sur le *Régime maigre*, les médecins les plus fameux, tant anciens que modernes.' He was a conscientious practitioner of his own eccentric doctrines, and it was perfectly consistent with his character, that 'loin d'imputer la mort du chanoine à la boisson et aux saignées, il sortit en disant, d'un air froid, qu'on ne lui avait pas tiré *assez de sang*, ni fait boire *assez d'eau chaude*.'

" The practice of *bleeding* was carried to a singular extent in France, and it was the fashion, at one time, to bleed on the opposite side to the part affected; if the pain was on the right side, they bled in the left arm, and *vice versâ*. Pierre Brissot produced a civil war in the medical world by writing against the custom, and, in the year 1600, was driven into exile, by edict of the University of Paris, for thus opposing the established practice.

Sir CHARLES SCARBOROUGH

" Was a man of great versatility of talents ; he wrote a 'Treatise on Trigonometry,' and a 'Compendium of Lilly's Grammar ;' gave lectures on mathematics at Cambridge, and on anatomy in London. His epitaph records that he was

> Inter Medicos Hippocrates,
> Inter Mathematicos Euclides.

He read the lecture founded by Dr. Caldwell, at Barber-surgeons' Hall, for many years ; where he was the first who attempted to account for muscular strength and motion on geometrical principles. He was a man of amiable manners and great vivacity of conversation. Seeing the Duchess of Portsmouth eat to excess, he said to her, with his usual frankness, 'Madam, I will deal with you as a physician should do ; *you must eat less, use more exercise, take physic, or be sick.*'

Dr. PITCAIRN

" Was a great enemy to quackery and quacks, of whom he used

to say that there were not such liars in the world, except their patients. A relation of his, one day, asking his opinion of a certain work on fevers, he observed, 'I do not like fever curers; we *may guide* a fever—we cannot *cure* it. What would you think of a pilot who attempted to quell a storm? Either position is equally absurd. We must steer the ship as well as we can in a storm; and in a fever we can only employ patience and judicious measures, to meet the difficulties of the case.'"

Turn we now to the second article in our list,—*Nugæ Canoræ;* and we are satisfied that our readers will agree with us in the correctness of our *guess*. It is the production, at any rate, of one who has lived much in the medical world, and no unobservant spectator of the vices and virtues, the feelings and failings of contemporary practitioners, possessing tact to "catch the manners living as they rise." In short, it is a pleasant *jeu d'esprit;* and we hail it as an omen, that in these "piping times of peace," the days of Garth, Goldsmith, and Darwin may be revived, and that the medical fraternity may again employ their *leisure* hours in amusements for which their education and intercourse with society so well qualify them.

After a humorous preface, in which the removal of the College of Physicians to Pall-mall East is lamented, the work, for very satisfactory reasons, is dedicated to the Presidents of the two Colleges and to the Master of the Company of Apothecaries, for the year 1927—and as a character in one of Foote's farces wishes he were to be born "fifty years hence," so should we like to have a peep at the "Clines and Coopers," the "Halfords and Warrens," of that day. We wish, with the author, that they may be as distinguished ornaments of their profession as those of our own.

That the old college should still be preserved for medical purposes, it is proposed to turn it into a "Medical Mausoleum," where the "Medical Fraternity are to be buried on the same terms as the Parisians are at Père la Chaise—and then follow the supposed Epitaphs of the present race of the "Medici." Due honour is done to learning and talents; while quackery, in all its ramifications, meets with just castigation. The names of Heberden, Turton, and Baker are noticed with the respect to which their virtues and acquirements entitle them.

Passing from these, we are introduced to an eccentric of the old school.

"SIR RICHARD JEBB, BART. M.D.

" Here, caught in Death's web,
Lies the great Doctor JEBB,
Who got gold-dust just like Astley Cooper;
Did you speak about diet,
He would kick up a riot,
And swear like a madman or trooper.

" When he wanted your money,
Like sugar or honey,
Sir Richard looked happy and placid;
Having once touched the cash,
He was testy and rash,
And his honey was turned to an acid.

" Sir Richard was very rough and harsh in manner. He said to a patient, to whom he had been very rude, '*Sir, it is my way.*' 'Then,' replied the patient, pointing to the door, 'I beg you will make *that your way.*' Sir Richard was not very nice in his mode of expression, and would frequently astonish a patient with a volley of oaths. Nothing used to make him swear more than the eternal question, 'What may I eat?—Pray, Sir Richard, may I eat a muffin?' 'Yes, Madam, the *best thing* you can take.' 'O dear! I am glad of that. But, Sir Richard, you told me the other day, that it was the *worst* thing I could eat!' 'What would be proper for me to eat to-day?' says another lady. 'Boiled turnips.' 'Boiled turnips! you forget, Sir Richard, I told you I could not bear boiled turnips.' 'Then, Madam, you must have a d——d vitiated appetite.'"

We cannot help bringing before our readers the following well-known " characters" of their day, and should have indulged in more ample quotations from these amusing " Epitaphs," were we not afraid of the imputation of " inappropriateness."

" *On a most venerable and highly venerated Surgeon, lately deceased.*

'Multis ille bonis flebilis occidit,
Nulli flebilior quam mihi.'—HOR.

" Of manners gentle, and in soul sincere,
Removed beyond this sublunary sphere,
Here lies an honest man!
Endued with caution, yet devoid of fear,
In practice dexterous, in judgment clear—
Excel him if you can!"

To this, we think, may be affixed the name of Henry Cline!

"CHARLES GOWER, M.D.

'Discours de bons mots!'

"Ye sons of humour, of frolic, and fun,
This stone will inform you that Gower is gone.
Poor Gower! eccentric, facetious, and funny,
Lik'd nothing so well as other men's money.
Alas! he is gone—'tis hard to say where,
The victim of mirth, imprudence, and care.
Where'er he is gone, his companions he'll smoke,
For, cost what it will—he will have his joke.

"'I knew him well, Horatio!'" exclaims our Author—"'a fellow of infinite jest!'—Chairman of the St. Alban's Club, where oft 'he set the table on a roar.'—And who did not know this eccentric oddity? Gower had considerable talents, but they were directed every way but the right. He made medicine a plaything, never being steady in professional pursuits. He wrote several singular books: one he entitled 'Auxiliaries to Medicine;' another, 'The Art of Painting;' both of which pourtray the character of their author. His unsteadiness led him into difficulties, and he died in obscurity."

"DALMAHOY.

'Thrice happy were those golden days of old,
When dear as burgundy p'tisans were sold.'

"Dalmahoy sold infusions and lotions,
Decoctions, and gargles, and pills;
Electuaries, powders, and potions,
Spermaceti, salts, scammony, squills.

Horse-aloes, burnt alum, agaric,
Balm, benzoine, blood-stone, and dill;
Castor, camphor, and acid tartaric,
With *specifics* for every ill.

But with all his specifics in store
Death on Dalmahoy one day did pop;
And although he had doctors a score,
Made poor Dalmahoy shut up his shop."

"HENRY REVELL REYNOLDS, M.D.

'Os placidum moresque benigni.'

"Here well-dressed Revell Reynolds lies,
As great a beau as ever!
We may perhaps see one as wise,
But sure a smarter never.

"Dr. Reynolds may be considered as the link between the ancient and modern costume of the Faculty: to the last, he wore a well-powdered wig and a silk coat. He was an excellent

specimen of a well-dressed and well-bred gentleman. As a practitioner he ranked in the first class, and he was one of the physicians who attended King George the Third during his afflicting and protracted malady.

"RICHARD GRINDALL, Esq.

'Eamus quo ducet gula.'

"Within this place Dick Grindall lies,
Who was a rare game chicken.
So, so, friend Dick, an old chum cries,
The worms have pretty picking!

No Surgeon better lov'd himself;
He lov'd old rum and brandy
As much as misers do their pelf,
Or children sugar-candy.

And as for eatables—in short,
He lov'd both roast and boil'd;
Fish, flesh, or fowl, of any sort,
If not by cooking spoil'd.

But though full well he lov'd good cheer,
It was a venial fault;
Since Reason's feast to him was dear,
Season'd with *Attic Salt.*

"He was an excellent surgeon of his day; that is, fifty years before Abernethy or Cooper was dreamt of. He was also a great oddity, but a perfect gentleman in his appearance and manner; never seen, by any accident, but in a well-powdered wig, silk stockings, and shoe-buckles. He practised in the City, when the city aristocracy resided within its walls, and Haberdashers' Hall, in the season, assembled all the wit, wisdom, and wealth of London merchants, in a sort of conclave of saltatory civic magnificos."

We just remember him, and that, after a long illness, he went round in his carriage to return thanks for "obliging inquiries," leaving his card, on which was written, "the remains of Dick Grindall."

The third and last work we have to notice, comes more legitimately before us, and is a novelty in medical literature—a sort of *Sketch Book,* containing much entertaining anecdote, that makes the information it contains extremely amusing.

The work is divided into three parts, as the alliterated title quaintly informs us—Mems., Maxims, and Memoirs. The first is a chronological record, giving, as it were, a "bird's eye view" of the most interesting events in the history of medicine, from the time of the conquest up to the

present century. The second consists of comments, or short essays, illustrative of some of the most important facts; and the third of biographical anecdotes.

Under the head of "*Medical Books*," we are presented with curious specimens of our earliest writers, with comments; but let the author speak for himself.

"One of the first of our English writers, is John of Gaddesden, whose 'Rosa Anglica,' was greatly esteemed, and he is favourably mentioned by Chaucer. John was a man to whom nothing came amiss; he had an anodyne necklace for fits, and an infallible cataplasm for gout; he was a dexterous bone-setter, and a good dentist. He was very assiduous in inventing lotions for ladies' complexions; and was complaisant enough to cut their corns; and as for those troublesome animalcules, which, in those days, used to infest *gentlemen's* heads, he had a most effectual method of destroying them; and in his celebrated book, he favours us with a whimsical cure for small-pox.—'Immediately after the eruption, cause the whole body of your patient to be wrapped in red scarlet cloth, or in any other red cloth, and command everything about the bed to be made red. This is an excellent cure. It was in this manner I treated the son of the noble King of England, when he had the small-pox; and I cured him, without leaving any marks.'

"Such was our countryman, John of Gaddesden, who deserves notice, moreover, as being the first English surgeon employed at court; and that the King (Edward III.) wrote a letter to the Pope in favour of him."

Speaking of Ardern's manuscripts, he observes—

"These manuscripts, though they are more ludicrous than luminous, are extremely well worth the attention of the surgical antiquary, from the numerous illustrations they contain of the mode and manner in which Ardern performed his operations; which, considering that he was an *improver* of surgery, gives us a glorious notion of what the art was previously to John's refinements, or those of [illegible]er Franks, whom he mentions with great praise."

"ANATOMICAL LECTURES.

"W[illegible] H[illegible] began his anatomical lectures, they were given [illegible] but as he lived at the period when Garrick was [illegible] discovered that he stood no chance with the [illegible] Garrick *lectured*, the *anatomical lectures* were neglected. In vain did the Doctor preach to the pupils on the immorality of attending theatres, and the impropriety of neglecting him; it [illegible] no avail; Romeo's apothecary and Dr. Last were the only [illegible] characters to spend the evening with, and for the rest, they thought Macbeth sufficient authority, to 'throw physic to the [illegible]!'

"For this reason, and for this reason alone, the anatomical lectures were afterwards given in the middle of the day.

"Dr. Hunter may be considered as the father of the anatomical schools of London, and he bequeathed a fame and character to his class, which has been supported with undiminished lustre to the present day. Previously to his time, very little had been done; Cheselden had given a few lectures—so had André, and Nourse; and Dr. Frank Nicholls gave what he considered a systematic course, and published a Syllabus of thirty-nine lectures. Dr. Maclauren and Dr. Marshal were also anatomical teachers. To the late Mr. Cline, however, and to Mr. Abernethy, we are indebted for the anatomical schools at two of our largest hospitals.

"Mr. Cline, it is true, found a place to lecture in, but it was his great talents and his high character, that brought it into notice, and subsequently, with Sir Astley Cooper, made it one of the first schools in Europe.

"To Abernethy is due the sole honour of establishing the Anatomical School at St. Bartholomew's, now second to none; and it is to the advantages arising from the hospital education of the metropolis, that London has become, within the last half century, the most distinguished seat of medical tuition in the world. Long may it flourish!

'Quicquid est laudabile, idem est beatum et florens.'—*Cicero.*"

"APOTHECARY.

"Apothecary, in its derivative sense, does not seem to allude particularly to the sellers of medicines. Αποθηκη is of very indefinite signification, *(Horreum,)* a market, shop, or repository, which may be used or applied to any other business. Chaucer and Pegge make it *Poticarry*, while some have derived it from *A-pot-he-carries*, intimating, that they used to carry the medicines themselves, as well as see them administered. 'Give me an ounce of civet, good apothecary,' says Shakspeare.

"The ancient apothecaries were called PIZOTOMOI, root-cutters; and root-cutters they may still be considered; at any rate, no one will deny to honest, herborizing Tom Wheeler, the character of a primitive PIZOTOMOΣ.

"That they may still be characterised by that appellation, their 'herborizing walks,' and their botanic garden at Chelsea, afford very creditable proofs; nor is there any circumstance in the history of the present worshipful society that reflects more honour on their zeal in promoting those branches of science, which appertain to their avocation, than the [illegible] and liberality with which, during the last two centuries, they have maintained their establishment at Chelsea.

"An active and intelligent member of their court has furnished them with a very interesting and ample memoir on the subject.

by which it appears, that this expensive design was commenced at a time when the society was without any disposable funds, when their hall was burnt down in the memorable fire, and when they were obliged to draw upon their own private pecuniary resources, to enable them to enter on an undertaking, 'whose principal design was honourable reputation, without any prospect of worldly advantage.'

"Previously to the establishment of this garden, there had been nothing of the kind, with the exception of a few private gardens, the most conspicuous of which were those of the celebrated John Gerarde, and the elder Tradescant; the former of these not then being in existence, and the latter in a state of neglect and ruin; and the locality of their position is now only known from the records of the times.

"There was, however, besides these, a small garden in Westminster, belonging to Mrs. Gape, the plants from which furnished the first specimens for the Chelsea Garden. It appears from Evelyn's journal, that he paid old Mrs. Gape's *medical garden* a visit in June 1658; whether he begged, borrowed, or bought any plants, does not appear; that he had a very fine garden at Sayer's Court, is well known; but that he lent it to that royal barbarian, Peter the Great, when he was studying ship-building at Deptford, is, perhaps, not so generally known, nor, moreover, the return this royal carpenter made to Evelyn's politeness, or the manner in which he showed his horticultural taste, in being wheeled through his landlord's ornamental hedges, and over his borders, in a wheel-barrow; a circumstance which is recorded in a letter to the then Secretary of the Royal Society.

"In France, the apothecaries were incorporated so early as 1484; but it was not till the reign of King James the First, when the metropolis abounded in dangerous empirics, who made and compounded many 'hurtful, false, and pernicious medicines,' that the Worshipful Society of Apothecaries were incorporated in London. Notwithstanding a charter was given them to correct these [illegible], it was found to be nugatory with respect to those who [illegible] members of the society; and, although they made repeat[illegible]tions to parliament, it is only within these very few years that their powers have been extended, and that they could legally enter the shop of any 'person or persons using the art and mystery of an apothecary, in any part of England and Wales, for the purpose of searching, surveying, and proving whether the medicines, wares, drugs, or any thing or things whatsoever, in such shop or shops contained, and belonging to the art or mystery of an apothecary, be wholesome, meet, and fit for the cure, health, and ease of His Majesty's subjects.'"

"TOBACCO.

'Tobacco's a physician,
Good both for sound and sickly;
'Tis a hot perfume,
That expels cold Rheume,
And makes it flow down quickly.'

"So says an old song, in an old play, and so said Dr. Ralph Thorius, and the learned Dr. Everard, who wrote a book, entitled 'Panacea, or a Universal Medicine, being a Discovery of the wonderful Virtues of Tobacco' (1659); and in the frontispiece of his book, the Doctor is represented with a pipe in his mouth. Dr. William Butler, styled, by Fuller, the Æsculapius of his age, was also a great admirer of tobacco, and that he might not smoke a dry pipe, he invented a medical drink, called 'Butler's Ale;' afterwards sold at the Butler's Head, in Mason's-alley, Basinghall-street.

"Sir Theodore Mayerne gives a curious specimen of his tobacco practice: 'A person applying to him with a violent defluxion on his teeth, Butler told him, that 'a hard knot must be split with a hard wedge,' and directed him to smoke tobacco without intermission, till he had consumed an ounce of the herb. The man was accustomed to smoke; he therefore took twenty-five pipes at a sitting. This first occasioned extreme sickness, and then a flux of saliva, which, with gradual abatement of the pain, ran off to the quantity of two quarts. The disorder was entirely cured, and did not return for seventeen years.'

"Ant. Wood says, that he was much resorted to, 'and had been more, did he not delight to please himself with fantastical humours.'

"Many singular stories are related of him, perhaps they are travelling stories, as may be conjectured, from the nature of the prescription, when he ordered a lethargic parson to [illegible] put into the warm carcase of a newly-killed cow!

"Fuller paints this humorist in striking colours, [illegible] observes, 'that he made his humorsomeness to become him; [illegible] some of his profession have rather aped than imitated [illegible] who had morositatem æquabilem, and kept the tenor of the [illegible]rliness to all persons.'

"The following extracts from *Letters from the Bodleian*, vol. ii., will give a notion of his *humour*, and of his mode of treating his patients.

"'Dr. Gale, of St. Paul's schoole, assures me that a Frenchman came one time from London to Cambridge, purposely to see him, whom he made stay two houres for him in his gallery, and then he came out in an old blue gowne. The French gentleman makes him two or three very low bowes downe to the ground; Dr. Butler whippes his legge over his head, and away goes into his chamber, and did not speake with him. He kept an old mayd, whose name

was Nell. Dr. Butler would many times goe to the taverne, but drinke by himselfe: about nine or ten at night, old Nell comes to him with a candle and lanthorne, and sayes, "Come home, you drunken beast." By and by Nell would stumble, then her master calls her "drunken beast;" and so they did "drunken beast" one another all the way till they came home.'

"'The Dr. lyeing at the Savoy in London, next the water side, where was a balcony look't into the Thames, a patient came to him that was grievously tormented with an Ague. The Dr. orders a boate to be in readinesse under his windowe, and discoursed with the patient (a gent.) in the balcony, when, on a signal given, two or three lusty fellows came behind the gent., and threw him a matter of twenty feet into the Thames. This surprise absolutely cured him.'

"'A gent. with a red, ugly, pimpled face, came to him for a cure. Said the Dr. "I must hang you." So presently he had a device made ready to hang him from a beam in the roome; and when he was e'en almost dead, he cuts the veins that fed these pimples, and lett out the black ugly blood, and cured him.'

"Butler must have been a man of abilities, for the Lord Treasurer Burleigh wrote to the President of the College of Physicians, desiring that Butler might be allowed to practice in London occasionally, and he was consulted, with Sir Theodore Mayerne and others, in the sickness that proved fatal to Prince Henry; and it is reported that Butler, at first sight of him, gave an unfavorable prognostic. The account of this case affords such an excellent notion of the consultations and practise of the doctors of those days, that I am induced to give it as stated in the 'Desiderata Curiosa.'

"*The Manner of the Sickness and Death of Prince Henry, 6th Nov. 1612.*

"'Dr. Atkins, a Physician of London, famous for his practyce, honestie, and learninge, was sent for to assiste the reste in the cure.

"'He got worse, whereupon bleedinge was again proposed by Dr. Mayerne and the favorers thereof, alledging that in this case of extremity, they must (if they meant to save his life) proceed in the cure, as though he was some meane person.

"'This was not agreed to, and next day, the Physicians, Chirurgeons, and Apothecaryes seemed to be dismayed, as men perplexed, yet the most part were of opinion, that the crisis was to been seene before a final dissolution. *This day a cock was cloven by the backe, and applyed to the soles of his feete.* But in vayne. Shortly after it was announced that all hope was gone. His Majestie then gave leave and absolute power to Dr. Mayerne, to do what he woulde of himselfe, without advise of the rest; but the Doctor did not it seems like this, "for hee, weighing the greatness

of the cure and eminencye of the danger, would not, for all that, adventure to doe any thinge of himself, without the advice of the rest, saying, that it should never be said in after ages, that he had kylled the Kynge's eldest sonne."

"'Bleeding was again proposed by Mayerne, but Doctors Hamond, Butler, and Atkins could not agree about it; instead of which they doubled and tripled the cordials.

"'Then came to assist the rest, Dr. Palmer and Dr. Giffard, famous physicians for their honestie and learninge. The result of this consultation was *Diascordium*, which was given in the presence of many honourable gentlemen.

"'All sorts of cardials were sent. *Sir Walter Rawleigh* sent one from the Tower.'"

"Mrs. MAPP.

"No part of surgery is supposed to be so easy to understand as *bone-setting;* it is regarded by a considerable part of the people as no matter of science, an affair on a level with farriery, as easily learnt, and like a heritage, to be transmitted from father to son; in short, the pretensions of these people are very like those of the man who set up as an oculist, *because* he had *lost an eye*, or the rupture doctor, who cured *bursten* children, *because* his grandfather and grandmother were both *bursten.*

"We are not without plenty of ignorant and impudent pretenders at the present day, but the celebrated Mrs. Mapp, the bone-setter of Epsom, surpasses them all. She was the daughter of a man named Wallis, a bone-setter at Hindon, in Wiltshire, and sister to the celebrated 'Polly Peachem,' who married the Duke of Bolton. Upon some *family quarrel*, Sally Wallis left her professional parent, and wandered up and down the country in a miserable manner, calling herself 'Crazy Sally,' and pursuing, in her perambulations, a course that fairly justified the title. Arriving at last at Epsom, she succeeded in humbugging the worthy bumpkins of that place so decidedly, that a subscription was set on foot to keep her among them; but her fame extending to the metropolis, the dupes of London, a numerous class then as well as now, thought it no trouble to go ten miles to see the conjuror, till at length, she was pleased to bless the afflicted of London with her presence, and once a week drove to the Grecian Coffee-house, in a coach and six, with out-riders! and all the appearance of nobility. It was in one of these journeys, passing through Kent-street, in the Borough, that being taken for a certain woman of quality from the Electorate in Germany, a great mob followed, and bestowed on her many bitter reproaches, till Madame, perceiving some mistake, looked out of the window, and accosted them in this gentle manner: 'D——n your bloods, don't you know me? I am Mrs. Mapp, the *bone-setter!*' upon which, they instantly changed their revilings into loud huzzas.

"That she was likely enough to express herself in these terms,

seems very natural from her origin and history; but that she should be on visiting terms with decent people, and keep quality company, is as unnatural. Mr. Pott, who wrote with the pen of a master, has noticed this in no very gracious terms:—'We all remember,' says he, 'that even the absurdities and impracticability of her own promises and engagements, were by no means equal to the expectations and credulity of those who ran after her; that is, of all ranks and degrees of people, from the lowest labourer or mechanic, up to those of the most exalted rank and station; several of whom not only did not hesitate to believe implicitly the most extravagant assertions of an ignorant, illiberal, drunken, female savage, but even solicited her company; at least, seemed to enjoy her company.'"

"TAR WATER.

"Bishop Berkeley, who brought this remedy into fashion, was greatly aided by the faith of the clergy, who preached it up in all quarters. Among these, none was more strenuous than Dr. Young, the author of the 'Night Thoughts.' 'They who have experienced the wonderful effects of tar water,' says he, 'reveal its excellencies to others. I say reveal, because they are beyond what any can conceive by reason or natural light. But others disbelieve them, though the revelation is attested past all scruple, because to them such strange excellencies are incomprehensible. Now give me leave to say, that this infidelity may possibly be as fatal to morbid bodies, as other infidelity to morbid souls. I say this in honest zeal for your welfare. I am confident, if you persist, you'll be greatly benefited by it. In old obstinate chronical complaints, it probably will not show its virtue under three months; tho' secretly, it is doing good all the time.'

"Such was the universality of its power, that it was good for man and beast, *and a sure remedy for the plague!*"

After this miscellaneous and amusing collection, we arrive at the Memoirs, which is not a dry, biographical record of birth, death, parentage, and education, but a lively sketch of characteristic particulars of eminent medical men. We will select a few of them.

"BUTTER.

"Mr. John Whitehurst (author of an ingenious theory of the earth) was the means of Dr. William Butter's settling at Derby, where he (Mr. W.) then resided. Mr. Whitehurst had met at Buxton with Lord Hopetown, who had asked him what physicians were at Derby, and upon his telling him, that there could not be a finer opening, as the two physicians there had both declined practice, his Lordship said it would be a good place for Butter; and shortly afterwards, the Doctor made his appearance loaded with recommendations, and among others, with one from Dr. Hope

to Mr. Whitchurst. Mr. W. was very civil to him, but before he had been a fortnight in the town, Butter came and complained, that he had not had a single patient. Mr. W. told him, that he could hardly expect any so soon, that he must be known a little, and so on, which so offended Butter, that ever afterwards he considered Mr. W. as his enemy. He was very rude and coarse in his manner, always averse to consultations, and used to say, that nobody but himself and Sir John Pringle knew any thing of physic. Among his patients at Derby were two brothers, opulent men, who lived together; one of them being dangerously ill, and attended by Butter, the other brother sent a messenger to Birmingham for two physicians, and then told Butter what he had done, and that he intended to have a consultation. Butter immediately went to the apothecary, and got some laudanum, of which he gave large doses to the patient, so that when the Birmingham physicians came, the patient was in a state of lethargy. They asked if he had been taking opium, but Butter denied that any had been given; it was accidentally discovered, however, by means of the apothecary, and from that time Butter, who was before in excellent practice, lost considerably in public estimation.

"A tailor at Derby, whom Butter had offended, once played him a trick. A curer of smoky chimnies came to Derby, and one day, when the tailor knew the Doctor was out of town, he called on the chimney-man, and told him that Butter had desired to have a smoky chimney cured, belonging to his best parlour; and had left positive orders that he should go to his house and set about it immediately. The operator accordingly went, delivered his message to Butter's servant, pulled out his utensils, and fell to work; and in a short time the marble slab, and other ornaments of the chimney, were down. Butter came in while he was engaged in this business; finding his parlour full of bricks and dirt and mortar, his fury was excessive, and his hatred to the tailor was ever after implacable. The story got wind in the town, and the boys in the street would sometimes talk about *chimney-doctors* as he passed.

"Butter lived close to a churchyard, and one day, seeing a grave-digger at work, he asked him for whom he was digging the grave—'For so and so,' said the grave-digger, naming the tailor who had so highly offended him, which so pleased the Doctor, that he gave the fellow a shilling. This occasioned a fresh laugh at his expense, as the tailor was in good health, and it was merely a piece of pleasantry of the grave-digger's. Butter and his wife lived in the most frugal manner, and never visited anybody. After he came to London, a lady of fortune, who had been his patient in Derbyshire, and wished to countenance him, invited him often to her table, till at length Butter brought in an account of fees for each visit."

"CADOGAN.

"Universal temperance in eating and drinking has been considered as particularly incumbent on a physician, in every period of his practice. It is a virtue he is frequently obliged to inculcate on his patients; and his doctrines will have little effect if they be not regularly exemplified in his own conduct.

"Dr. Cadogan, however, thought it right *to try all things*, and considered it his duty to speak *experimentally* on both sides of the question, to qualify himself to say, in the language of Dido,—

'Non ignara mali miseris succurrere disco.'

"Thus, dining one day at a College dinner, after discoursing most elegantly and forcibly on abstinence, temperance, and particularly against pie-crust and pastry, he is reported to have addressed a brother M.D. in the following terms: 'Pray, doctor, is that a pigeon pie near you?' 'Yes, sir.' 'Then I will thank you to send me the hind-quarters of two pigeons, some fat of the beef-steak, a good portion of the pudding-crust, and as much gravy as you can spare!'"

"BLAIR.

"'We physicians were always politicians,' was a favourite expression of Warren's, but nevertheless, there are very few instances of medical men embroiling themselves in political troubles.

"Dr. Patrick Blair, however, who was in the rebellion of 1745, got himself into Newgate, and was condemned to be hanged. In the British Museum are several of his letters to Sir Hans Sloane, written in prison, soliciting his intercession, and in one of them he writes, 'If you come towards Newgate, I hope you will favour me with a call.' Dr. Martyn, the professor of Botany at Cambridge, supped with him in Newgate the night previous to his expected execution. Blair had been all along confident that he should be reprieved: Dr. Martyn said, he sat pretty quietly till the clock struck nine, and then he got up and walked about the room; at ten he quickened his pace; and at twelve, no reprieve coming, he cried out—'By my troth! this is carrying the jest too far!' The reprieve, however, came soon after, and in due time a pardon. Blair went afterwards, and settled at Boston in Lincolnshire, where he practised till his death."

"SIR WILLIAM DUNCAN.

"Sir William Duncan once met Dr. Thomas Reeve, when the latter was President of the College, and insisted that his name should not follow Reeve's, because he was physician to the king. Reeve asserted his dignity as president, and the consequence was, that each wrote his own prescription (the same they had agreed to) and gave it to the apothecary.

"There are many instances of medical etiquette being carried to a great extent, but polite etiquette in a sick room was perhaps

never exceeded by the following exhibition of it, between the Duke of Ormond and a German Baron.

"The Duke of Ormond and a certain German Baron were both considered models of pride and politeness. When the Duke perceived that he was dying, he desired that he might be seated in his elbow chair, and then, turning to the Baron, with great *courteousness*, he requested that he would excuse any unseemly contortions of feature, as his physicians assured him, that he must soon struggle with the last pangs. 'My dear Lord Duke,' replied the Baron, with equal *politeness*, 'I beg you will be on no ceremony on my account!'"

"BAILLIE,

"Not Matthew Baillie, but an Irish gentleman who had been rejected by the College, called the next day on Dr. Barrowby, who was one of the censors, and insisted upon his fighting him. Barrowby, who was a little puny man, declined it. 'I am only the third censor,' said he, 'in point of age—you must first call out your own countryman, Sir Hans Sloane, our president, and when you have fought him and the two senior censors, then I shall be ready to meet you.'

"Many medical duels have been prevented by the difficulty of arranging the 'methodus pugnandi.' In the instance of Dr. Brocklesby, the number of paces could not be agreed upon; and in the affair between Akenside and Ballow, one had determined never to fight in the morning, and the other that he would never fight in the afternoon. John Wilkes, who did not stand upon ceremony in these little affairs, when asked by Lord Talbot, 'How many times they were to fire?' replied, 'Just as often as your Lordship pleases; I have brought a *bag of bullets and a flask of gunpowder*.'"

"WOODVILLE.

"Dr. Joseph Adams, who was much with Woodville just before his death, used to relate several traits of his firmness and seeming unconcern with respect to death. Woodville lived in lodgings at a carpenter's in Ely-place, and Adams, a few days before his death, advised the matron of the Small-pox Hospital to invite him to have a bed made up there, that he might be better attended to: this she did, and Woodville accepted it. He observed to Adams, the next day, that he was a poor man come to die at the hospital, and he remarked, that some of those who called on him flattered him with hopes of his getting better. 'But I am not so silly,' he said, 'as to mind what they say; I know my own case too well, and that I am dying. A younger man with better stamina might think it hard to die; but why should I regret leaving such a diseased, worn-out carcase as mine?'

"The carpenter with whom he lodged had not been always on the best terms with him; Woodville said he should wish to

let the man see that he died in peace with him, and as he never had much occasion to employ him, desired he might be sent for to come and measure him for his coffin. This was done; the carpenter came, and took measure of the Doctor, who begged him not to be more than two days about it; 'For,' said he, 'I shall not live beyond that time;' and he did actually die just before the end of the next day. He got between one and two thousand pounds by his Medical Botany, and with the money bought a small estate, which he left to his natural daughter, being all the property he possessed."

We happen to know this fact, and moreover, that the Doctor was playing at chess when the carpenter was introduced to measure him for his wooden surtout. "Mr. ——," said the Doctor, "you come at the proper season, for *my game is nearly finished!*"

The work is embellished with three etchings, which remind us that Mr. Wadd not only uses the pen, but the pencil, with facility and taste. His published works afford ample proof of his power of *illustrating* morbid anatomy, but we happen to know of some *unpublished folio proofs* of equal merit. To his fair fame as a surgeon, by the works we have just noticed, he may add the reputation of being one of the most vivacious literary *illustrators* of his art.

On Tic Douloureux.

Sir,

Presuming that popular and domestic medicine may occasionally find a niche in your Journal, I beg to offer a few remarks upon the above complaint, which has lately become, as it would appear at least, singularly prevalent; and as I address myself to general readers, I shall avoid all learned terms of art, and minute descriptions requiring them. The genuine tic douloureux is usually considered as a morbid affection of the nerves of the face, very commonly attacking the circumference of the orbit, and producing frequent and violent paroxysms of excruciating pain; the disease, however, varies considerably in intensity, and sometimes bears the same name when attacking other parts; it frequently occurs under the integuments of the head, and may or may not be attended with external tenderness. Though opiates relieve the pain, they are ineffectual as to its cure. Peruvian bark, in

various forms, has sometimes afforded relief, and preparations containing the metallic tonics, more especially the oxides of iron, have been regarded as giving more permanent and beneficial assistance. Local remedies are of very uncertain utility, and electricity and galvanism have generally done more harm than good. The division of the nerves has been resorted to, but never with permanent, and often not even with temporary benefit. The cause of the disease is unknown, and though sometimes organic derangement would appear to excite it, no plausible source of the mischief can usually be discovered. The patient's principal solace is that the disorder frequently wears itself out, and as far as my experience goes, the less we rely upon individual remedies, the better—the main thing being strict attention to the general health, and especially to the state of the stomach and bowels. These remarks apply to the genuine Tic Douloureux; but it has of late years been the fashion in physic to give that alarming name to a variety of painful affections, resulting from very various causes, by which much needless uneasiness has been given to the patient, and which has often led to erroneous and even mischievous systems of practical treatment. As cases of this kind are of every day occurrence, a short notice of them can scarcely be inappropriate to a Journal, the chief object of which is to familiarize every branch of science.

Rheumatic affections of the head and face often put on the appearance of Tic; like it, they come on at short intervals, and are limited to a small space; there is, generally, more or less of external tenderness, sometimes confined to spots upon the face and scalp, not larger than a shilling; at others, more diffused. More or less of this is usually attendant upon habits subject to chronic rheumatism, and it not uncommonly is the leading feature of the complaint. The internal use of opiates and sudorifics, especially small doses of Dover's powder, warm fomentations, and keeping the head, especially at night, wrapped up in flannel, are sovereign remedies.

But the most common cases of painful affections, mistaken for Tic, are those which occur in nervous and irritable persons, and especially amongst men of business, statesmen, lawyers, merchants, over-studious persons, and all whose minds are

occasionally exercised beyond their powers, who are subject to reverses of fortune, or sudden changes in the posture of their affairs, and who are constant objects of public attention, praise, or censure. For a time, the constitution, if a good one, bears up against such wear and tear, but as you advance, one or other symptom of a shattered nervous system appears, and this, more quickly and certainly, where the body has been pampered by too good living, false spirits excited by indulgence in wine, and fatigue relieved by narcotics, instead of sleep. Among the host of disordered affections to which such persons are liable, violent local nervous pains are most common, but they are invariably relieved by such means as contribute to quiet the mind and invigorate the body. Abstinence from business, retirement into the country, regular hours, plain food, moderate exercise, and avoiding excitement, are here certain remedies, and indeed the only ones, but they are unfortunately not always easy of attainment, and sometimes altogether unattainable. I have, however, mentioned these cases, to enjoin an early attention to the overhanging evil, and to criticise its improper treatment. I would, upon the first point, enjoin early attention to the first symptoms, and when they appear let the individual seriously ask himself whether it be worth while to gain a little more money, glory, or honour, or renown, at the expense of all future comfort, and a painful, wearisome, and probably shortened existence; or whether such apparent advantages had not better be at once conceded, and the host of evils, which will almost certainly ensue, warded off by a timely retirement? I could illustrate this subject by reference to many individuals, especially in the legal and medical professions, some of whom are harassing themselves to death by over-exertion, whilst others (I regret to say but few) are preserving a healthy constitution, by sacrificing a certain share of fame and emolument: the exceeding folly, too, of persevering in business, when neither mental nor bodily powers are adequate to the exertion, might here be animadverted on, but I must, for the present, waive such topics, and return to the treatment of those nervous pains called Tic Douloureux, which are of such common occurrence in the cases alluded to. These will certainly give way under

that quiet and retirement which has been above recommended; but it is really provoking to see such means so commonly neglected, and the unfortunate patients tormented by blisters, fomentations, and galvanism, and their already debilitated stomachs further overpowered by gigantic doses of powdered bark, rust of iron, and other (in such cases) equally ineffective and hurtful medicines. I write to warn against them.

I have spoken of Peruvian bark as a remedy in tic douloureux. Where the painful affection so called, let it arise from what cause it may, assumes an intermitting form,—and nothing is more common than to have it coming on at stated periods, generally one violent attack in the twenty-four hours,—in such, as in other similar cases, bark has often been effective; but of late, sulphate of quinine has very properly been substituted for it; and as this extremely curious and valuable medicine is now in every one's hands, and even finding its way into family medicine chests, a few words respecting its use, or rather abuse, may not be here misplaced. I would first remark, that it is too commonly given in over-doses: it then produces thirst, and a white tongue, and, what is remarkable, it excites in most people that uneasy sensation of fulness about the stomach, which is generally complained of after a large dose of powdered bark, and ascribed to the indigestible nature of the large quantity of inert and insoluble woody fibre in which that substance abounds. For these reasons sulphate of quinine is too often laid aside in cases where, if properly and judiciously administered, it might prove of important service; instead of three or four grains, or even more, repeated every four or six hours, let a grain be given once a day; and if it agree, and occasion require, let this dose be repeated twice or thrice daily, either in the form of pill or solution. I prefer the latter; two drachms of tincture of orange-peel being used as the solvent, and diluted afterwards with half a wine-glass of water. It is not meant here to insinuate, that in obstinate agues, and other disorders, large doses of quinine are always improper, but to enforce the occasional mischief which they produce, and by which the medicine is unjustly brought into distrust and disrepute.

Decayed teeth are fertile sources of pains and twitches

about the facial nerves and muscles, analogous to Tic; and great irritation from inflamed membranes of some cavity in the upper jaw has also occasioned them. I knew a person who suffered six months from such an attack, and for whom a physician prescribed, in the course of that period, some pounds of carbonate of iron. Symptoms then ensued, for which a course of sarsaparilla was ordered, but it was of no avail. Mercurials were then given, with manifest mischief. The extraction of the second grinder effected a permanent cure; its roots were connected with a cavity of fetid discharge, which had no sooner vent, than all the symptoms disappeared.

Without exceeding the limits which I have set myself, I cannot proceed further in these remarks; but I hope enough has been said to quiet the apprehensions of some invalids who suffer themselves to be exceedingly alarmed at the name given to their complaint, and to be dosed with large quantities of useless medicines, which rather aggravate than relieve it. In many of these cases, the less that is done the better; in all of them, careful reference must be had to the real exciting cause; and, in addition to the other circumstances adverted to, a strict attention to diet must be enforced, and more than ordinary watchfulness exerted over the state of the stomach and bowels: plain roast and boiled, and no grease or piecrust in the former; and for the latter, an occasional blue pill and a tea spoonful of Epsom salt.

MEDICUS.

Remarks on some Quadrupeds supposed by Naturalists to be extinct. By John Ranking, Esq.

THERE is not any part of the creation more interesting to mankind than the gigantic classes of quadrupeds. In them, we are able to contemplate the power of the Creator of all things, in one of the most magnificent exercises of his will. Such, however, is the limit to this kind of knowledge, that there is probably not any one class, even of the largest quadrupeds, all the *species* of which are, or possibly ever can be, known to the student of natural history. More than half of

the surface of the earth is still undiscovered by the civilised portion of its inhabitants: regions as extensive as Europe, in Asia, Africa, and America, are, at this time, either wholly unknown or undescribed.

The imperfection of history is such, that the most civilised ancient states of the world have left little behind but what may be called fragments of their annals. If we include the Gothic age, as it is called, from the fifth to the fifteenth century, there are not less, out of the fifty-eight centuries which the earth is said to have existed, than forty of them which may be termed a blank, as far as regards profane and natural knowledge.

The period assigned to the Deluge is seventeen centuries after the creation, or upwards of four thousand years past. There are not any known real historical annals that can contest this event, and the natural state of the earth offers abundant proofs of its reality. Under all these considerations, the fossil remains of elephants and other large quadrupeds, known to have been employed or slain by the Romans and Moguls, may justly be considered as independent of any relation to that catastrophe, and in no wise concerned in the discussion. Established truths are rather disturbed and weakened by arguments which are open to refutation.

The time is not distant when it will be generally acknowledged that all those kinds of quadrupeds, the remains of which have been found at the very places mentioned in history, are still in existence; a fact which, when proved, will be of infinitely greater interest as it regards so grand a portion of nature, than the single supposition that they are all extinct, because we are not acquainted with the exact species which corresponds with many of the fossil kinds frequently discovered: this being the foundation on which such a conclusion is principally built.

Naturalists have endeavoured to prove, that such bones are found where they could only have been placed by the Deluge: but the changes in the surface from deposits by rivers, earthquakes, and imperceptible alterations from the accretion of vegetable matter, and from dust, volcanoes, digging of mines, wells, canals, foundations, and other disturbances of the soil,

are such as cannot be observed or registered; and a few lines will prove how difficult and uncertain this part of the question remains to this day.

"In quarrying limestone at Aix, in Provence, A. D. 1788, under eleven strata, separated from each other by a bed of sand and clay, at the depth of forty-five feet, the surface was covered with shells. The stones of this bed being removed, under a stratum of argillaceous sand, stumps of columns and fragments of stones, like the quarry, half wrought, were found; and also coins, handles of hammers, and a board, one inch thick and seven feet long, broken, but all the pieces there, and could be joined; it was like the boards used by quarry-men, and worn in the same manner. The pieces of wood were changed into *agate**."

"On sinking a well on a hill near Tobolsk, sixty-four *fathoms* deep in the earth, an oaken beam was found; it was quite black, and not round but shaped †."

"At Watlington-park, Oxfordshire, at fifty or sixty feet depth, many whole oaks, hazel-nuts, a stag's-head and antlers, were found, and on the same spot two Roman urns ‡."

"In Oxfordshire there is a tumulus which has become a perfect mount of stone."

"Ralph, the brother of Earl Widdrington, showed me many *human* bones taken from whole skeletons, with British beads, chains, iron rings, and brass bits of bridles, dug up in a *quarry* at Blankney, Lincolnshire, which was probably plain mould when these old corpses of the Britons were interred: and I saw many *human* bones and armour, with Roman coins, fibulæ, &c., found in a *stone*-pit in Hunstanton-park, Norfolk, belonging to Sir Nicholas L'Estrange §."

Very numerous instances could be added, in order to prove that the local circumstances, when skeletons of these quadrupeds are found, are not of a nature to disprove the *historical* origin of fossil bones. From the highest authority we learn, that the "bones of species which are apparently the same with

* Count Bournon; Phil. Mag., vol. lvii., p. 458.
† Strahlenberg, p. 405.
‡ Dr. Plott's History of Oxf., p. 161.
§ Phil. Trans. Abridged, vol. iv., part ii., p. 273.

those that still exist alive, are never found except in the latest alluvial depositions, or in the fissures of caverns and rocks, in places where they may have been overwhelmed by debris, or even buried by man*.

Thus it appears that a comparative view of the exact *species* now living, with that of the fossil remains, is what we must depend on to decide whether the fossil kinds may not be still in existence.

With respect to the very numerous theories of the earth, the last, by Werner, has been confidently quoted in opposition to the writer's historical proofs †. But Werner himself, before his death (in 1817), tacitly acknowledged that it is not a tenable doctrine, and which is clearly indicated by the compilers of REES's *Cyclopedia* ‡, although it is generally allowed to be the best extant. This hypothesis was formed on a circumscribed view of the strata in Saxony, but it is found to be quite inapplicable, in America for instance §. To account for fossil bones of elephants, &c., being found high in the north, the American author who discovered this defect in the geological doctrine, conjectures that those large quadrupeds may have migrated, like the buffalos, during the change of seasons. This notion, however, would not apply to Asia, the native countries of those animals being well supplied with leaves or other food the year round.

With these prefatory remarks some historical proofs are offered, for the probability of the following animals found in a fossil state, not being of extinct species, beginning with the

ELEPHANT.

"On sinking the foundation for a mill, near the side of a small brook in the Bishop of Kilmore's lands, at Maghery,

* Cuvier, Theory of the Earth.

† In the American Quarterly Review, published at Philadelphia, March, 1827. Art. "Fossil Remains."

‡ Titles, "Werner," "Fletz," "Transition."

§ See two dissertations on the Geology of the U. S. of N. America, by W. M'Clure, Esq., in the Transactions of the Amer. Phil. Soc., new series, vol. i., Philadelphia, 1818. This gentleman had entertained a different view in the previous volume; but after eight years' experience, in Europe and America, he had the philosophical justice, boldly to amend his former opinions.

eight miles from Belturbet, in the north of Ireland, A. D. 1715, four large teeth were found, with a piece of the under jawbone and part of the skull of a young elephant. The teeth were more solid and petrified than when in a natural state."

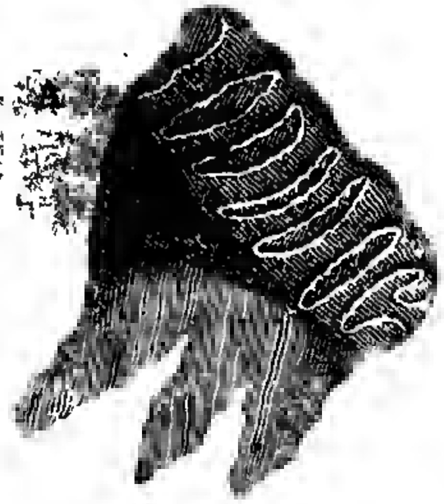
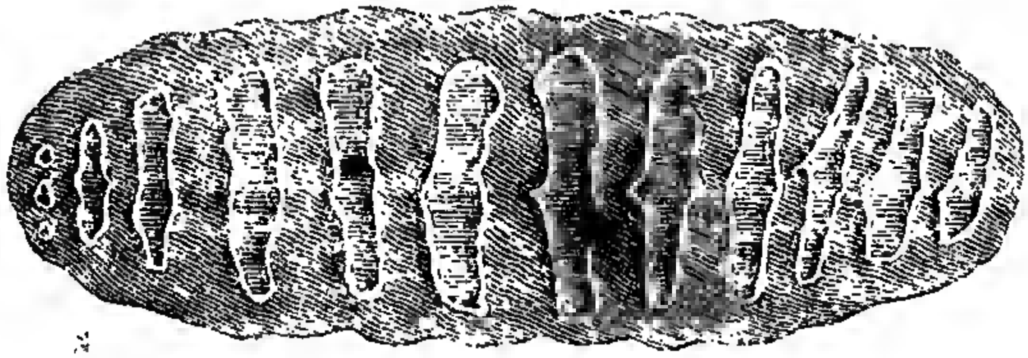
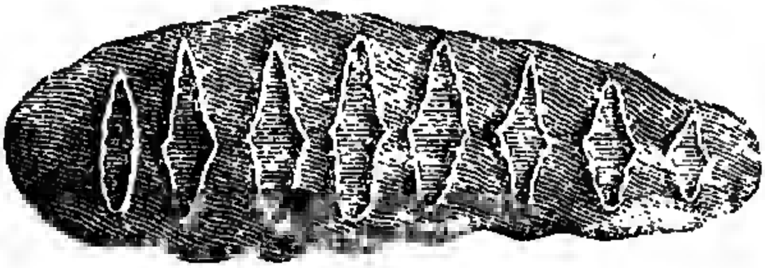


Fig. A is one of the above grinders. B is a fossil grinder in the possession of the Royal Society. C is the grinder of an elephant between 10 and 11 feet high, the entire skull of which was then in Westminster*.

B



C



It is thus apparent that two *fossil* elephants are of the same species as those now in existence.

It is not improbable that the Maghery animal was conveyed to Ireland as a present, or for exhibition. "Fiacra, son of Eacha Moymedon, was mortally wounded at the battle of Caonry, which was fought A. D. 380, wherein he was victorious against the army of Momonia, (Munster). On his return to Hy-mac-uais, in *Meath*, he died of his wounds. His funeral

* See Phil. Trans. Abridged, vol. iv., part ii., p. 236 to 245, and Camden's Brit. Gough's Ed., 1789, vol. iii., 604.

leacht was erected, and on his tomb was inscribed his name in the Ogham character*."

We here find that the native sovereign of the northern part of Ireland resided in Meath, the borders of which county are not many miles from the place where the elephant was found. It was at about the year of the battle of Caonry that Maximus, the emperor in Britain, aspired to be master of the Roman empire. Finding the union of the Scots and Picts prevented his peaceable possession of Britain, which was a great obstacle to the execution of his project, he persuaded the Picts to join their forces to his, on the promise of giving them the lands of the Scots. The Scots were thus overpowered, and were forced to fly to Ireland and the adjacent isles. The Scots, being assisted by the Irish, invaded the north, and were driven back to Ireland by Maximus, at the head of his troops. The emperor threatened to invade Ireland, and punish the Irish; but the dread they had of the presence of a Roman army, induced them to grant Maximus his own terms, which, in order to conciliate all parties, were moderate†. Now it is by no means impossible that the British emperor, on this conciliating occasion, sent this very elephant to his Irish majesty. Tacitus observes, that Agricola (three centuries before Maximus) received an expelled petty king of Ireland into his protection; that in manners the natives vary little from the Britons; and that *the ports and landings of Ireland are better known, through the frequency of commerce and merchants, than those of Britain*‡.

The Mastodon.

This quadruped is now known not to differ from the elephant, except in the form of the grinders, and has probably been called by the name of elephant by the Romans. Remains of the mastodon have been found mixed with those of the elephant, in Europe, Siberia, and America; and for the following

* Essay on the Antiquity of the Irish Language, by Lieut. Col. Vallancey, 8vo., London, 1818, p. 12.

† See Gibbon, ch. xxvii., Zosimus, b. iv., Rapin, b. i., Wars and Sports, ch. xiii.

‡ Life of Agricola.

reasons there is every probability of this animal being in existence.

Captain C. S. Cochrane, in his *Journal in Colombia*, vol. ii., p. 390, relates that numbers of the *carnivorous* elephants have been seen feeding on the plains at the foot of a ridge of mountains, at Choco, in New Granada. "Part of the foot of a mastodon, with five nails attached, was found in a cave, with a tooth, by a savage west of the Missouri: it was very fresh, and perfectly resembling that of an elephant: it was obtained of a Mexican, who had purchased it of a native *."

"The native Americans describe the elephant as still existing in the northern parts of their country (the Missouri)."—Mr. Jefferson's *Notes on Virginia*, p. 57.

Many bones of the mastodon were found in the county of Wythe, Virginia, with a mass of half-ground branches, roots, and leaves, enclosed in a kind of sack, supposed to be the stomach, in the midst of them; so as to leave no doubt that they were substances which the animal had devoured, and among them were distinguishable the remains of some plants known in Virginia †. Teeth of the mastodon have been found in Little Tartary, (for five centuries possessed by the Moguls,) in Siberia, near the Oural mountains, and one at Harwich, in England ‡.

There have been brought from Ava, found on the left bank of the Irawaddy, in N. lat. 20° to 21°, near the wells of petroleum, in narrow ravines, sand-hills, beds of gravel, ironstone, and calcareous breccia, evidently a *diluvial* formation,—fossil bones, shells, and wood. Bones of the mastodon, equal in size to those of the Ohio, a grinder $16\frac{1}{2}$ inches in circumference, a humerus, measuring 25 inches round the condyles, with several

* Parkinson, vol. iii., letter 26. Mr. P., relates that Baron Cuvier inclines to doubt the authenticity of this account; but Capt. Cochrane's testimony *now* renders it very probable to be correct. It is very worthy of remark, that the wild elephants in America are found, as reported, at Choco, and west of the Missouri; and that Mango Capac and Montezuma's ancestor, by the traditions, landed at Cape St. Helen's and Culiacan,—as if some elephants had been let loose, or had escaped and betaken themselves to perhaps the nearest thick forests, and have remained there undisturbed.

† Rees's Cyclopedia, Addenda, "Mastodon."

‡ See Parkinson, vol. iii., letter 26, p. 367.

grinders and bones of younger individuals, and fragments of tusks: fossil molares of the rhinoceros, resembling two species of a genus named by Cuvier Anthracotherium: bones like an animal of the horse kind: remains of crocodiles, supposed to be the gavial, or long-nosed alligator of the Ganges, (not now known in the rivers of Ava.) The fossil bones were upon or near the surface, more or less exposed, not decomposed or rolled, and are of animals that died there. The bones are petrified, and deeply coloured with iron, the substance siliceous and very hard. The blocks of wood are larger than the trees growing there, but it is not known if they are of the same kind. "An idle notion is entertained by many, that these fossil remains have been generated by a petrifying quality in the water of the Irawaddy*, but I think they are the result, as elsewhere, of one of the last catastrophes; in fact, the remains of a former world, before man was called into existence."—*Morning Herald*, Sept. 14, 1827.

Bones of the mastodon have been found in Europe, mixed with menagerie collections, which cannot possibly be attributed to any other origin than that of sports of the amphitheatre. They are found in western Siberia, which was conquered by Sheibani, Genghis Khan's grandson, A. D. 1242, and held 300 years, and whose first capital was at Tiumin †, on the river Tura, near the Ural mountains, where the remains of the mastodon were found. Ava was conquered by the Grand Khan Kublai in 1272, in a battle with the king of eastern Bengal, in which there were a thousand elephants ‡. The places where they have been found in America correspond with history and tradition so faithfully, as to assist the other numerous proofs of Mexico and Peru having been conquered by the Moguls, in the year 1283, and the bones of the mastodon are there found, as well as remains of *elephants, precisely like those of Siberia* §. With regard to the tooth found at Harwich, the

* Duchat, an author of unquestioned credit, has seen recent wood petrified into flint by the water of a river in Ava. Rees's Cyc., "Wood."

† Levesque, Hist. de Russie, vol. vii. 244.

‡ Wars and Sports, p. 263.

§ Conquest of Peru, ch. x. It is somewhat curious that, when Pyrrhus for the first time brought elephants into Italy, the Romans gave them the name of Lucanian bulls; and that the Americans call them

British kings Cuneboline* and Arviragus had representations of elephants on their coins. The bones of elephants, rhinoceroses, and crocodiles found in Ava are not, as those found in Europe and Siberia, what are termed *extraneous* fossils; the same kinds of animals being natives of the spot in Ava. The one like the horse cannot be ascertained; but the kings of Pegu, in former times, had camelopards, and, therefore, probably, zebras in their *calichars*, or parks; they also had *unicorns*, ostriches, and rein-deer†. Timur Khan, grandson of Kublai, who invaded Siberia with such powerful armies, resided at Tali, in Yunan, N. lat. 25° east of the Irawaddy‡.

The writer is of opinion that all those fossil bones found in Ava are of species still in existence: they may have floated down from more northern parts, the river in question being as long as the Ganges, said to be navigable into China; and has its source in Thibet,—(see Rennell's *Memoir*, p. 217.) According to the hypothesis of the writer, Montezuma's ancestor was a Mongul grandee from Assam; and mastodontes' remains have been found in Mexico, and those beasts are, as above related, supposed to be found alive near the Missouri.

This is the first instance the writer has met with of similar bones not being *extraneous*; and is, therefore, a remarkable fact, which excites the strongest suspicion that their species are still living. Ava is a new world on a small scale, and this collection of bones will, very probably, at no distant date, lead to positive proof of the existence of other quadrupeds, now conjectured by naturalists to be extinct. With respect to the local position, it is in all probability the old bed of the river, as

big bulls in their traditions. It is probable that both people compared them with the largest beast known to them; as elephants, if indigenous in America before the arrival of Mango Capac and Montezuma's ancestor, would have been extremely numerous, and have had a proper name.

* Shakspeare spells this name Cymbeline; Milton writes Kymbeline, which is probably the true pronunciation: see his History, 8vo. 1695, p. 62.

† Wars and Sports, p. 269.

‡ Id. p. 506. The Burmans eat elephants. The writer was at Dacca in 1794, when some Burmese troops invaded the Chittagong frontier. An expedition, under Colonel Erskine, was sent against them; and on the return to Dacca of Colonel Boujonnar's battalion, the officers told the writer that they found in the stockade the skeleton of an elephant, which the Burmans had devoured.

the beds of those in Asia change in a wonderful manner.—(See *Rennell*, p. 255.)

A skeleton of an elephant or mastodon, for it is not known which, was found in a tomb in Mexico, which had evidently been built on purpose.—(*Clavigero*, vol. i., p. 84.) No authority whatever dates the foundation of Mexico earlier than A. D. 1324. The Aztecs advanced from Culiacan, when they took possession of the marshes, and founded Mexico: other Aztecs had preceded them who had arrived by land; but the writer hazarded a theory[*] that *Montezuma's* ancestors had, like those of the Natchez and of the Incas, arrived in America by *sea* with elephants, under Mango Capac; and he has had the satisfaction to find a confirmation of his conjectures in a *decade* written by Peter Martyr, the Milanese, (employed by Ferdinand V., King of Castile and Arragon, and who died in the year 1526,) addressed to Adrian VI., who had been co-regent of Spain with Cardinal Ximenes. "Montezuma spoke thus to Cortez:—We have heard by our ancestors that we are strangers. A certain great prince, in *ships*, before the memory of all men living, brought our ancestors unto these coasts; whether voluntarily or driven by tempest it is not manifest; who, leaving his companions, departed into his country, and, at length returning, would have had them to have gone back again. But they had built houses, and joining themselves with the women of the country had begotten children, and had settled. Wherefore our ancestors, having chosen a senate and princes to govern the people, refused to go, and he departed with threatening speeches. Never any appeared unto this time who denied the right of that captain and commander. We think, therefore, that the king who sent you derived his descent from him, and all the kingdoms which we possess are yours[†]." It is impossible to know clearly what the allusions to the return of the great commander may mean, but whatever it be, it does not change the date. As the Mexicans considered Cortez to be a child of the sun, the *great prince* must have been a descendant from Genghis Khan; and *thence* the

* Conquest of Mexico and Peru, p. 288-301.

† Hakluyt, vol. iv., p. 558; and Conquest of Peru and Mexico, ch. vii.

terrors and submission of Montezuma and the Mexicans, who had always dreaded such a visit.

The Aztecs had sojourned in Culiacan and other places, from the date of the arrival of the ships, till they proceeded to Anahuac. The foundation of Tenochtitlan (or Mexico) having been in 1324, and the first king, Montezuma's ancestor, elected in 1377; therefore, the empire, when Montezuma died, had lasted only 144 years; and this calculation is from the most authentic documents known, that is, the pictures in Purchas's collection. In Harris's *Voyages*, vol. ii., p. 97, Montezuma is said to have told Cortez, that it was only a century since they had been settled where they were, meaning, probably, that it was not *two* centuries.

Thus an elephant being found in a tomb in Mexico, and others in tombs in Siberia, is an additional argument to the strong ones already produced, for the Mexicans being the Moguls blown from the shores of Japan, A. D. 1283, which appears irresistible; and also that mammoths and mastodontes are not extinct, being found either living or fossil in all the places in America, which agree with the traditions on that subject, and with the histories of China and Japan*.

THE TAPIR.

The Tapir was supposed to be peculiar to the New World: two fossil species, one of them gigantic, have been found in

* A Roman coin is said to have been discovered recently among the Indians in America, which has justly created surprise; but others have been found long ago. Bishop Hakewill's book is dated, A. D. 1635: he says, "Marianus Siculus, in his history of Spain, reports that certain coined pieces of gold, engraved with the image and inscription of Augustus Cæsar, were found in the American mines; thereby inferring that those countries were *then* discovered." p. 310. Batou, the cousin of Kublai, both grandsons of Genghis, had conquered Russia, ravaged Europe to the Adriatic, and died on his march to Constantinople, in 1256. His successor also ravaged as far as Constantinople, (P. de la Croix, p. 387.) Mango (so spelt by Du Halde, ii., 251, and Maundeville, p. 275; Manku by Tooke, Russ. Emp., ii., 13) was brother to Kublai, who is considered by the writer to be the father of the first Inca, and there is nothing more probable than that he and other Moguls on the Japanese expedition may have possessed Roman coins, the plunder of Hungary, Poland, Dalmatia, and the Greek empire, as far as the capital.

France, Germany, and Italy *. The remains of a tapir being found at Florence, with those of other quadrupeds usually exhibited by the Romans, was an unaccountable fact, till it was known, through Sir Stamford Raffles, that the tapir exists in Sumatra. We know that the Romans carried on a commerce with India, which employed *one hundred and twenty ships annually*, and that they had the power of being supplied with all the animals of those regions, by means of country ships, which traded to the ports of Musiris and Barace, those which the Romans frequented. Moreover, the author of the *Periplus*, p. 36, describes *Sumatra*. It appears, therefore, evident that the Romans procured tapirs from that island, if they be not inhabitants of Africa. The British king, father of Caractacus, had a tapir on one of his numerous coins †; which may be reckoned among many other proofs that the ancient Britons were not quite so ignorant and barbarous as is generally, but unjustly, imagined. The discovery of this tapir shows how little is yet known even of those countries in which Britain has, for a length of years, had establishments. The tapir is probably what the natives have reported as a *river-horse*, a much more appropriate name for it than for the African beast. "The descriptions of the hippopotamus," says Baron Cuvier, "by Herodotus and Aristotle, are supposed to have been borrowed from Hecatæus of Miletus, and must have been taken from two very different animals, one of which is the true hippopotamus, and the other the *antelope gnu* of Gmelin ‡." Now, as it appears that the Indians described by Herodotus by the name *Padæi*, is an exact account of the *Batta* in Sumatra,—(Dr. Leyden thinks them the same word, as the Indo-Chinese pronounce B as P §,)—it is rendered probable that that island was known to the Greeks, long before the Romans possessed Egypt. On these grounds, I venture a conjecture that Aristotle and Herodotus alluded to the tapir, which is *amphibious*, but the gnu is not. The tapir is probably the *küda-ayer* of Sumatra, and the *conda-aijeer*, or *rivier paard*, of the

* Cuvier, Theory of the Earth, p. 257.
† Conq. of Peru, &c. plate iv.
‡ Theory of the Earth, p. 67.
§ Herodotus, Thalia xcix. Rees's Cyc., "Sumatra."

Javans.—(See MARSDEN's *Sumatra*, third edition.) With respect to the *gigantic* tapir, it is as probable that those regions (apparently less known to moderns, as regards zoology, than to the Greeks and Romans) may contain gigantic tapirs as ouran-outangs, near eight feet high, so lately discovered.

UNICORN.

Many reasons have been given, in another place *, to prove the probability of the existence of the unicorn, since which the following description of two has been met with.

"On the other part of the temple of Mecca are parks or places enclosed, where are seen two unicorns: they are shown to the people as a miracle; and not without good reason, for their rareness and strange nature. One of them, which is much higher than the other, is not much unlike a colt of thirty months of age: in the forehead groweth one horn, in manner right forth, of the length of three cubits. The other is only one year of age, and like a young colt: the horn of this is of the length of four handfuls. This beast is of that colour of a horse called weasel, and hath a head like a hart, but not a long neck, and a thin mane, hanging on one side. Their legs are thin and slender, like a fawn or hind: the hoofs of the fore feet are divided in two, much like the feet of a goat: the outer part of the hinder feet is very full of hair. This beast seemeth wild or fierce, yet tempereth that fierceness with a certain comeliness. These unicorns were given to the Sultan of Mecca as a most precious and rare gift. They were sent him out of Ethiopia by a king of that country, who was desirous by such a present to gratify the Sultan †."

So lately as the year 1799, a Mahomedan African prince is said to have sent two of them to Mecca.—(REES's *Cyclopedia*, "Monoceros.") Bell of Antermony describes one which was killed in Siberia, near the *Irtish*, in 1713. Tamerlane slew unicorns and rhinoceroses on the frontier of *Cashmere*, (*Sheref-eddin*, b. 4., ch. xxx.) and there have recently been *reports* of unicorns in *Nepaul*, which are rendered more probable to be

* Wars and Sports, p. 335.

† Travels of Lewis Vertomanus to Egypt, Arabia, &c., A. D. 1503, in Galvano's collection. Hakluyt, vol. iv., p. 162.

the truth, by those references of Mr. Bell and Sherefeddin to countries not very distant.

The British king Cuneboline had also the unicorn on his coins, and the figure of the animal is very similar to the above description *. The writer is, therefore, of opinion that these now described are the real oryx mentioned by Aristotle, Pliny, and other ancient authors †.

HIPPOPOTAMUS.

The remains of this beast have been found in England at the residences of the Romans, *viz.*, near London, Colchester, and York; and not any in Ireland or Scotland. They have also been found in Italy mixed with great numbers of the bones of other beasts known to have been exhibited by the Romans. This animal is not known to inhabit any country but Africa. Two were caught near Damietta, A.D. 1600. They are known to inhabit Abyssinia, Bornou, the Cape of Good Hope, Senegal, and they were met with in great abundance by the two vessels, the Sion, of 200 tons, and St. John, of 50 tons, which sailed above nine hundred miles up the river Gambia, A.D. 1620, employed by Sir Wm. St. John ‡. The inference is, that they inhabit the whole of that vast continent, and that it is most probable the number of species is as great as that of elephants; and that the fossil kinds not having been brought from the same country as the living individuals with which they have been compared, has induced naturalists to suppose them extinct. An elaborately grand Roman pave-

* Wars and Sports, p. 354.

† See Cuvier's Theory of the Earth, p. 80. Wars and Sports, p. 335. With regard to the unicorn, Camper has remarked, that "if this animal was ruminant and cloven-footed, it is certain that its frontal bone must have been divided longitudinally into two, and that it could not possibly have had a horn placed upon the suture." This remark by Camper, when we consider how nature adapts every thing to its purposes, cannot stand as a real objection to the existence of the oryx. The most eminent naturalists have been wrong in some of their conjectures. John Hunter pronounced the mastodon to be a carnivorous beast. Buffon, after frequently considering the bones of the mammoth, conceived them to belong to a beast six times larger than the biggest elephant; and Muller was of opinion that it must have been 105 feet in height, and 133 in length! So little capable is any human being to judge what nature does, or can do!

‡ See Relation of Master Wm. Jobson in Purchas, vol. ii., p. 921.

ment was dug up at Roxby, in Lincolnshire, upon which is represented Orpheus, surrounded by an elephant, lion, boar, dog, wolf, stag, and another, which appears to be the hippopotamus *.

TURTLE. TORTOISE.

"A beautiful fossil sea-turtle has recently been discovered, and, by the perfect substitution of all the organic parts as well as its locality, may be considered an interesting remain of a former world. It is encrusted in a mass of ferruginous limestone, and weighs 180lbs. The spot on which it was found is in four fathoms of water, and is formed of an extensive stratum of stones, called the Stone Ridge, about four miles off Harwich harbour; and is considered to be the line of conjunction between the opposite cliffs of Walton and Harwich. It is in the possession of Mr. Deck, of Cambridge †."

A fossil turtle was found near Harwich, embedded in a solid block of cement-stone; another large stone, when broken, was found to contain "nearly the whole of a *human* skeleton ‡."

Fossil sea-tortoises have been found in the environs of Brussels, in the environs of Maestricht, at the village of Melsbroeck and in the mountain of St. Peter, in the state of Glaris and in the vicinity of Aix; they differ in species from any of those at present known §.

There is not any of the extraneous fossil remains more probably of Roman origin than tortoises. "The beds, the doors, and pillars of the houses of the Greeks and Romans, were decorated with tortoise-shell. In the reign of Augustus, this species of luxury was at its height in Rome ||. Bruce says, the Egyptians dealt very largely with the Romans in this elegant article of commerce; Martial relates that beds were inlaid with it; Velleius Paterculus observes, that when Alexandria was taken by Julius Cæsar, the magazines were so full of this article, that he at first proposed to make it the principal ornament of his triumph; as he used ivory afterwards when triumphing for his African victories ¶."

* Conq. of Peru, p. 450.
† New London Literary Gazette, Oct. 13, 1827, p. 303.
‡ Common Sense Newspaper, No. 60.
§ Cuvier's Theory of the Earth, p. 291.
|| Shaw's Zool., III. pt. 1. Rees's Cyc. "Tortoise." ¶ *Ibid.*

Cuneboline and his son Arviragus having had the elephant, tapir, and unicorn on their coins; and as the first was brought up at the court of Augustus *, there is every probability of their having possessed tortoises at Harwich, the port of the capital of the British king.

SPECIES.

With regard to elephants, the number of species appears to be very great, even with the extremely limited knowledge we possess. The writer saw three distinct kinds captured in one keddah at Tippera, when he was there during Mr. Corse's residence at that place, and who has described them. Some African females have tusks as large as the males, but it is not known to be so in Asia. Le Vaillant mentions a race of elephants which never have tusks. Two Ceylon elephants were found to differ in the shape of the jaws, and another is mentioned by Baron Cuvier, which is dissimilar to any that had been seen †.

The Camelopard now at Paris differs in many essential anatomical characters from the kind at the Cape of Good Hope ‡.

The Romans and Moguls crossed the species and genera of different animals. The crocotta was between a dog and a wolf; the crocuta between a hyæna and a lioness §. The Moguls cross the breed of dogs with leopards, the best of which are those of Hezereh and Tesheen in Cabulistan; and some are so brave that they will attack a lion ||. Four towns near Babylon were exempted from any other tax than the maintaining of dogs which were supposed to be produced between the tiger and bitch ¶. We thus may perceive how impossible it is to be certain of a fossil species being extinct because we are not acquainted with it.

* Milton's Hist. 8vo. p. 62. † Cuvier, Ossemens Fossiles, p. 185.

‡ Ed. New Phil. Journal, Sept. 1827, p. 390. Here is a direct instance, that if a fossil *Egyptian* camelopard had been found, it would, like elephants, &c., have been pronounced to be an *extinct* species, the modern specimens being from South Africa.

§ Pliny, b. viii. || Ayeen Akbery, vol. i. p. 242.

¶ Herodotus, Clio, cxci. We may conjecture that *tiger* has been written for *leopard*, a frequent error.

Ptolemy Philadelphus, in a procession at Alexandria, had twenty-four thousand Indian dogs, a camelopard, a white bear, and twenty-four chariots drawn by elephants, twelve by lions, seven by oryxes, eight by ostriches, four by wild asses, and five by buffaloes *. Bajazet, in the fourteenth century, had twelve thousand dog-keepers. The immensity of wild beasts slaughtered by the Persians, Moguls, and Romans, would be incredible, were it not attested by so many different authorities; and with regard to the Romans, no author mentions a less number than five thousand of every description slain at the opening of the Coliseum. These sports having been in vogue all over the Roman empire for so many centuries, the fossil bones which have been found are but few indeed. In Britain there were at least five amphitheatres; at Sandwich, Dorchester, Silchester, Caerleon, York †. In France, at Paris, Cahors ‡, Vienne, Arles, Orange, Autun, Treves, Nismes, Poitou §, and Bordeaux. In Spain, at Seville, Tarragona, Merida, and Saguntum. In Italy a great number. The popularity of monarchs and statesmen depended on their power to indulge the people with these cruel sports. Commodus is said to have been one of the most dexterous marksmen: he always had with him Parthians, to teach him archery, and Moors, to perfect him in throwing the dart. He ran with all horned animals, except bulls, and smote them unerringly as he pursued. Lions, panthers, and other fierce beasts, he ran after in the Peridrome, and darted at them from above with never-failing effect, whether he aimed at the forehead or the heart. With arrows, pointed like a half-moon, he would cut off the heads of the Mauritanian ostriches, while their wings were

* Montfaucon, vol. iii. p. 179.

† Augustan History, "Severus," p. 253. "Wherever Caracalla wintered, or but intended to winter, they were constrained to erect amphitheatres and cirques for public games, and those within a while were taken down again."—Hakewill's Apology, p. 443. Caracalla was three years at York; and Spartian, in his Life of Severus, relates, that among other omens just before that emperor died, (at York,) three figures of Victory, which stood upon the platform near the throne, were blown down while the *games of the circus were celebrating*. There was a Roman road from York to Whitby (Dunus Sinus), and Kirkdale is about half way between the port and the capital.

‡ Rees's Cyc. "Cahors."

§ Marquis Maffei, p. 260.

expanded to aid their speed, and they continued their course for a time without their heads. He would expose a prize-fighter to the attack of a panther, and strike the beast dead before it could fasten its teeth on the man. A hundred lions have been sent out of the dens, and all killed by him with such certainty, that they lay close together, not a dart failing *.

Domitian had been equally notorious in these grand sports in the Amphitheatre.

> "What scene sequestered, or what rude renown,
> Sends no spectator to the imperial town?
> The Rhodopean hind now tempts the plains,
> And tunes from Hemus his Orphean strains.
> The Sarmat, Cæsar, hies, thy works to see;
> And gives the steed he swills † to share the glee.
> They come, who first the rising Nile explore;
> And they who hear remotest ocean roar.
> The Arab hasted, the Sabean flew;
> And the Cilician own'd his native dew.
> With tortured tresses, here Sicambrians gay;
> There Ethiops, bristling in their diverse way.
> 'Mid various speech, but one glad voice we find,
> That hails thee father of converg'd mankind ‡."

As for the Romans themselves, according to Juvenal, these amusements seem to have been preferred to all others.

> "Could you the pleasures of the Cirque forego,
> At Fabrateria or at Frusino,
> Some villa might be bought, for what will here
> Scarce hire a gloomy dungeon by the year §."

Had the fossil animals died, or been killed by natural accidents, the skeletons would generally have been found entire, but for the most part they are scattered and broken, and are often mixed with bones of animals resembling the species of the present time ‖. In the vicinity of Orleans in France, a *fossil* roe, of a living species, was found in *limestone*, along with the bones of the *palæotherium*.

Instances have occurred of bones being found, in great numbers; and, many feet deeper, other heaps of bones of elephants and wild beasts; but as many amphitheatres were built

* Herodian, "Commodus."
† The Tartar opens a vein of his horse and drinks the blood.
‡ Martial (Elphinston's, p. 19) on the Sports of Domitian.
§ Satire iii.
‖ Cuvier. Theory of the Earth, pp. 89 and 263.

with wood, and as the games were exhibited for about six centuries, those structures would require to be often renewed, and the old bones would thus be covered over with earth. Britain was invaded or visited by about twenty emperors, or those so high in importance as to become emperors of Rome; and York was the head-quarters of the Roman empire during the residence in Britain of Severus and his two sons and co-emperors, Geta and Caracalla *. All the *collections* of fossil bones are found at the head-quarters of the Romans, or near the several amphitheatres in the island. Bones of elephants which have been found in France and Italy in fifteen places, are so faithfully accurate to the road over which Hannibal and Asdrubal with fifty-two elephants marched †, *and Hannibal's (thirty-seven) all perished before his arrival at Thrasymene*, that no theory whatever can stand in competition with such historical conviction ‡. If the bones found on Hannibal's road be not those of his Getulian elephants, are we to conclude that the remains of the beasts lost two thousand years ago have totally perished; but that other bones of elephants, many thousands of years older, have been preserved upon the same spot, although some of them are found quite near the surface? At Plaine de Grenelle, a fossil elephant was dug up, and at that place there stood a Roman amphitheatre §. The great numbers of elephants then used in warfare may be judged of, by Metellus having captured upwards of a hundred in the battle of Palermo, where many besides had been killed ‖; and accordingly fossil bones have been found, there and also at Syracuse, where there was an amphitheatre. In Spain, thirty-nine elephants were slain at Munda, in the battle fought between the two Scipios and Asdrubal. At the bridge of Manzanares, and at Toledo, fossil remains of elephants have been

* The emperors had their families and the whole Roman court with them. The celebrated Julia Domna, and her sister Julia Mesa, were there during those three years. See De Serviez, Roman Empresses, vol. ii. p. 239.

† Passage des Alpes par Annibal, d'après la narration de Polybe. Comparée aux récherches faites sur les lieux, par J. A. De Luc. Génève, 1818.

‡ Wars and Sports, p. 295.

§ Gibbon, ch. xix. p. 177.

‖ Catrou, vol. ii. p. 591.

dug up; and at these very places Hannibal and Asdrubal defeated one hundred thousand Carpetani, many of whom were trodden to death by their forty elephants *.

If we glance at the sports of the Mongols, what a treasure for an osteologist might be found at Termed in Sogdiana, where the army commanded in person by Genghis Khan were four months occupied in enclosing an immense circle, till all the wild beasts were driven (without one escaping, under pain of death to the soldier who failed in his duty, but who was not allowed to kill the tigers, lions, &c.) into a spacious plain, where they were slaughtered by the Grand Khan and all the Imperial princes and military commanders, till they chose to permit the soldiers to end the destruction †. How many fossil *species* might be discovered there, of which naturalists have no knowledge! The Persians are said to have slaughtered as many as fourteen thousand beasts on a like expedition ‡. So long have these amusements existed, that Hushing, king of Persia, B. C. 865, *bred dogs and leopards* for hunting §.

Besides the fossil remains which have been found of numerous quadrupeds, named by the Romans in their sports, they employed the following, bones of which have not been detected: —indian dogs, white bears, camels (one found), dromedaries, camelopards, wild asses, zebras, quaggas, oryxes (unicorns), Ethiopian sheep, Arabian sheep, the crocotta (bred from a dog and wolf), crocuta (from a hyæna and lioness), little dragons, ostriches. The gnu was known to the Romans; and probably the nyl-ghau and the om-kergay (quite harmless, and the size of a rhinoceros). In this list several of the fossil kinds described as the ancient wild beast with a thick skin (palæotherium), and the beast without weapons, or unarmed (anoplotherium), may be found, and also those of the genus canis, and a carnivorous beast ‖.

Such is a short notice of this most extensive subject, to which the writer's attention has been attracted by the concurrence of

* Livy, b. xxi. ch. v.; b. xxiv. ch. xlii.

† De la Croix. Hist. of Genghis, b. iii. ch. vii.

‡ Sir John Chardin, vol. ii. 33.

§ Sir William Jones, vol. v. 588. The above may possibly mean a cross breed of the two beasts, which we find is still practised in Cabulistan, as related in the Ayeen Akbery.

‖ See Rees's Cyc. "Strata."

historical relations with the locality of fossil remains. It is offered for the consideration of the reader, not in a spirit of controversy, but with a desire to ascertain an important truth in natural history, whether his speculations be confirmed or refuted. Whichever way a decision is awarded, it will add to the interest attached to zoological pursuits, and the reader will be, by these remarks, enabled to form a judgment whether the laborious and ingenious works which have been published, since the conviction that elephants are not human giants, (a notion seriously maintained so recently as in Clavegero's *History of Mexico,* written since that of Robertson) are descriptions of the quadrupeds of a *former world*, or of the world which is now in *existence. It is necessary to remark that these particular researches relate only to animals connected with Roman and Mogul history;* and if it should be conceded that it may justly be inferred, that quadrupeds hitherto deemed extinct are still to be found in the undiscovered parts of Africa, Asia, and America, not half of either region being yet scientifically known, it will give an interest to zoology and osteology ten fold more attractive than a blank and unsatisfactory hypothesis of their having all perished *before the creation of man*, as is often alleged. It is perhaps the most remarkable circumstance in literature, that naturalists so rarely allude to the astonishing number of beasts slain in the Roman games, although the list of them is, generally speaking, so similar to that of the fossil remains. Erroneous notions concerning fossil bones, those of elephants, in particular, being the most plentiful, began in very early ages when they were considered to be human; and James the First (of Britain) sent Lord Herbert of Cherbury to Gloucester, to ascertain if a skeleton, dug up at that place, was really that of a giant. There were found mingled with it horns and bones of oxen and sheep, and the tusks of a boar. Lord Herbert, Dr. Clayton, and the celebrated Harvey, thought the bones were those of one of the Roman elephants; and Bishop Hakewill received a letter from my lord of Gloucester, mentioning that "he was *not confident* that the grinder was the tooth of a man."* This discovery, perhaps, put an end in England to the notion of giants' bones.

* Hakewill's Apology, p. 229.

The next fanciful origin was, that these fossil remains were those of an extinct monster, called Mammoth by the native Siberians, their name for the walrus; but which was transferred and confounded with the bones of whales, elephants, and buffalos, found in that country, and such erroneous opinions will long be entertained in those quarters.

The diluvian origin was imagined by many to be the true one, but later careful examinations proving that the animals died on the spot where they are found broken, and the bones scattered about, that hypothesis could not in such instances be maintained, and recourse was had to the supposition, that Britain was in former ages a tropical country; but the mixed fossil remains, being those both of hot and cold climates, and of beasts peculiar only to Africa, or to Asia, this theory appears to be quite as objectionable as the others. The last, and the most specious, of all the hypothetical proofs of the origin is, that the teeth not often corresponding with those of the living specimens which have been seen, they must be the remains of extinct quadrupeds. There are, perhaps, fifty large regions where elephants abound, and the teeth of *very few indeed* of the animals of those countries have yet been seen. This last appears to be, defective as it is, the strongest objection that can be urged against the historical origin; and the few remarks in this essay will contribute materially to weaken this remaining hypothesis. The reader who feels any interest in zoology will, by their means, be assisted in his endeavours to untie or cut this gordian knot. After he has decided either that these beasts are in existence, or all extinct,

> "In his reflections, then, what scenes shall strike!
> Adventures thicken! novelties surprise!
> What webs of wonder shall unravel there*!"

Description of a cheap and portable Instrument for enabling Young People to acquire a knowledge of the Stars, or determine their situation in the Heavens. By S. Lee, Esq.

There is no science, the study of which tends so much to enlarge the mind as Astronomy. It opens to our view the grandest examples of Almighty power, wisdom, and beneficence—the

* Young. Night VI.

contemplation of which fills the soul with reverence and affection for the great Author of nature, and banishes all narrow and superstitious notions respecting him.

The cultivation of this science, therefore, cannot be too strongly recommended to the attention of young people. The eager curiosity and avidity for discovery which so peculiarly distinguish that period of life, when the reasoning faculties begin to develope, is peculiarly fitted for its reception—and, accordingly, amongst the better-educated classes of society, the elements of this science are generally considered as a necessary branch of instruction—though commonly limited to a mere *dogmatic* explanation of the Copernican system, and the use of the globes.

But this superficial mode of instruction, though sufficient to enable any one to understand the real motions of our planetary system, and explain the apparent motions which must result from them, is hardly sufficient to satisfy inquisitive reasoning minds, since it leaves them ignorant of the means by which the distances, magnitudes, and orbits of the planets and comets were first discovered; and how, if lost, a knowledge of them might be recovered from observations alone.

The most pleasing methods of instruction will generally be found the most efficient. It is impossible for any one who has had the least experience in teaching not to have perceived, that one practical application of science makes a deeper and more lasting impression on the mind of a learner than a thousand theoretic propositions.

An accurate knowledge of the fixed stars is the first step to practical astronomy; it is, in fact, the alphabet of the science. By the rising, southing, and setting of these bodies, astronomers are enabled correctly to measure time; and from their apparent altitudes, to determine the latitude of places on the surface of the globe, whilst the permanent situations which they maintain with respect to one another, furnish them with so many marks by which to trace the course of the sun, moon, [illegible] through the heavens. Such were the data which [illegible]rnicus and Newton to unravel the seeming irre[illegible] their apparent paths, and explain the beautiful sim[illegible]eir real motions.

[illegible] instruments usually had recourse to for this purpose

are, celestial globes, planispheres, and atlases, but none of these afford such ready and certain means of finding or identifying particular fixed stars, as at first might be expected from them.

The Globe possesses the great advantage of being easily rectified to the place of observation and adjusted to the exact hour of the night. It likewise exhibits all the stars in their proper situations of altitude and azimuth;—but the constellations being delineated on a convex surface, and viewed from without, whilst the heavens appear to us a concave viewed from within, the groups of stars are seen reversed, a circumstance which occasions no small degree of perplexity to a learner.

Planispheres and atlases exhibit the constellations as they appear to the eye when on the meridian, but in a position very different from that which they assume when removed far from it. In short, except the pleiades and a few remarkable groups, it is difficult to recognise a constellation in every position, without great practice and continued observation.

The Equatorial furnishes the best and readiest means of discovering or identifying any particular star, but the great price of this instrument, and the complicated nature of its adjustments, render it unfit for learners.

The instrument which we are about to describe, is in its principle the same as the Equatorial, though not pretending to any thing like the same degree of accuracy; but it has this advantage over it,—its adjustments are more simple and obvious, consequently, better adapted to the capacity of learners; and it can be afforded at a very moderate expense, the price not exceeding that of a common globe.

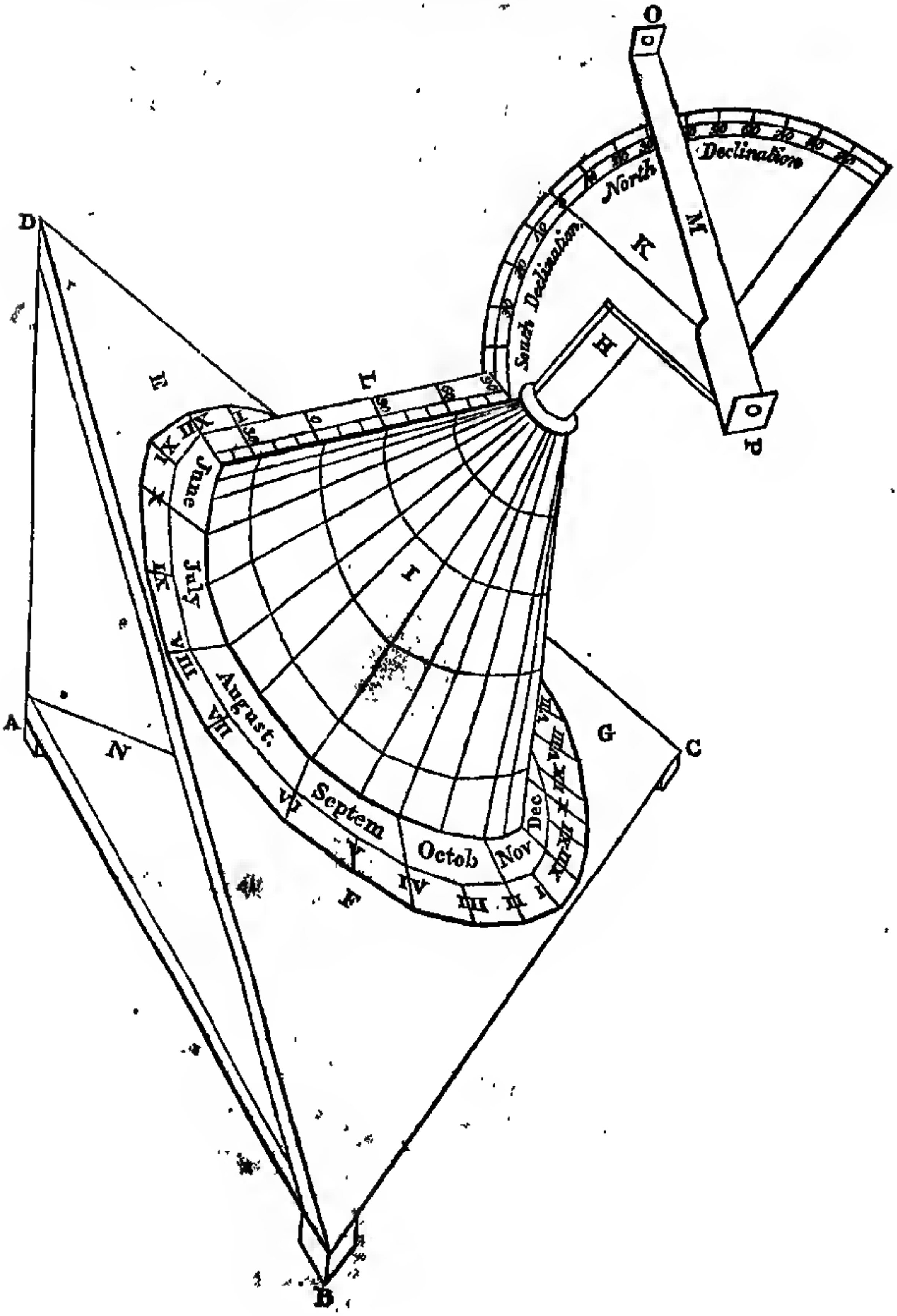
A, B, C, D, is the stand of the instrument, composed of three triangular pieces of wood glewed together, so that the plane of the upper piece, D B C, makes with that of the lower piece, A B C, an angle equal to the co-latitude of the place it is intended for.

On the upper piece, D B C, is described a circle, E F G, the circumference of which is divided into twenty-four hours, and every hour into twelve parts, equal to five minutes each.

From the centre of this circle, and perpendicular to the

plane on which it is described, rises a pillar, the top of which appears at H above the cone I, which can be made to revolve upon it as an axis.—On the surface of the cone is delineated the principal stars visible in England, and the lower edge is divided into 365 parts, representing the days of the year.

On the top of the pillar is fitted a segment of a circle, K, of



about 140°: viz. 90° of north, and 40° of south declination, which may be made to revolve upon the pillar as an axis independently of the cone. To this part is attached the scale, L, divided into degrees of north and south declination, corresponding to those on the semicircle,—and so contrived as in every situation to touch lightly the surface of the cone.

To the declination circle K is attached the alidade M, which may be set to any degree required, and serves as an index to direct the eye of the observer to any object, which may be viewed along the edge of it, or through the small holes in the sights O P.

Having described the several parts of the instrument, it only remains to shew the use of it; which will be best explained by means of a few problems.

Problem I. To adjust the instrument.

The instrument being made for the place of observation, need only to be placed on a perfectly horizontal stand, and with the line joining the hours XII and XII on the circle, in the direction of the meridian: the former of these adjustments may be verified by means of a small level applied to the stand at N, in the directions B C and A N successively. If found incorrect in either position, let a piece of card be put under that foot (A, B, or C) from which the bubble is found to recede, and let this operation be repeated until the bubble rests in the middle, in both positions.

The instrument may be brought into the meridian by the assistance of a magnetic needle fixed to the frame at N, or on the opposite side, or more correctly by means of the sun, provided the time be exactly known, thus:—

Set the index M to the sun's declination, turn the circle K round its axis, till the scale L points to the hour and minute on the circle E F G. Then if the instrument be correctly placed, the sun will be seen through the sights O, P, or what is the same thing, the light admitted at the hole O will fall on the hole P. If not, the instrument must be turned about till this effect is produced.

The instrument being once carefully adjusted to the meridian on any immoveable stand, such as the sill of a window,

the top of a post, &c., lines may be drawn on the stand in the direction of the sides A B, A C, or B C, by means of which it may at any time be replaced with little trouble.

PROBLEM 2. The instrument being correctly placed and levelled, the next operation will be to adjust the conical projection to the day and hour of observation.

Turn the cone round till the day of the month on the circle, at the bottom of it, coincides with the hour and minute on the circle E F G.

Example.—To adjust the cone for the 15th January, at twenty minutes past nine at night. Turn the cone till the 15th January on the circle attached to it coincides with IX h. 20 m. P. M. on the circle E F G.

PROBLEM 3. The cone being adjusted, and any star proposed, to find its place in the heavens.

The cone remaining at rest, turn the declination segment K till the scale L cuts the proposed star on the projection; note its declination on the scale and set the index M to the same degree on the segment K, when the index will point to the star, which, if the adjustments have all been correctly made, will be seen through the sights P, O.

Example.—To find the star Aldebaran; look for Aldebaran on the projection, bring the scale L to cut it, and you will find it against 16° north declination. Set index M to 16° N. P., and look along the edge of it, or through the holes P, O, and you will see the star.

PROBLEM 4. Having observed a star in the heavens, to find it on the projection.

Set the cone as accurately as you can to the day and hour, then turn the declination segment round, and elevate the index till you can see the star through the sights P, O. Note the declination at the segment K, cut by the index M, and against the corresponding degree of the scale you will find the star on the projection.

If no such star can be found, you may then conclude that it is a planet, or a new star.

By this means the place of the moon, of a planet, or a comet, may be noted down, from time to time, and their apparent paths traced out.

PROBLEM 5. To find the hour of the day by the sun.

Turn the segment K, and elevate the index M, till the sun is seen or shines through the sights O, P, and the scale L will point to the hour and minute on the circle E F G.

PROBLEM 6. To find the hour of the night by means of a star.

Direct the index M to the star, so as to be seen through the sights P, O; then laying hold of the scale L, to keep it in that position, turn the cone till the star on the projection is cut by the scale, when the day of the month on the circle at the bottom of the cone will coincide with the hour and minute on the circle E F G.

This instrument, though not capable of extreme accuracy, might, by means of careful workmanship, and the addition of a small telescope, be made sufficiently so for finding stars in the day time; but such a one as that now described will answer all the purposes of a learner, and enable very young people to acquire a correct and extensive knowledge of the stars in a very short time.

The surface of a cone has been adopted for the projection, in preference to that of a globe or planisphere, having been found, after repeated trials, the figure best suited to the nature of the instrument.

An Introduction to the Comparative Anatomy of Animals, compiled with constant reference to Physiology, and elucidated by twenty copper-plates. By C. J. Carus, M.D., &c. Translated from the German, by R. T. Gore, Member of the Royal College of Surgeons in London.

IF we except Sir Everard Home's splendid work on comparative anatomy, we have no original treatise on that subject which deserves notice; and even Sir Everard's lectures must rather be considered as a series of essays on detached parts of that branch of science, than as a regular and systematic view of it. We have long been acquainted with the work of Dr. Carus, and have always considered it as a laborious and accurate epitome of the principal facts and authorities in the study to which it relates. From the immense field of inquiry which it embraces, it is necessarily complicated, and

in some places a little obscure, but it is entirely free from those speculative and hypothetical wanderings which are too characteristic of the German school of physiology; and though it contains some systematic notions in which we cannot acquiesce, and a few new words, not the most harmonious in the world, it may very safely and properly be recommended to the student as a text-book, and to the proficient as a work of reference. The plates by which it is illustrated are upon an economical scale, sometimes rather too small to be distinct, but they are otherwise accurate and carefully drawn; and we are aware that it is impossible to obviate these objections without incurring such expense as would probably render the work inaccessible to those readers for whom it is principally compiled.

Mr. Gore has assiduously and faithfully executed the difficult task of translation, and has added no inconsiderable quantity of new and important matter in the form of notes, rendering the English work more complete, and upon many points much more satisfactory than the original.

Experiments to determine the Comparative Value of the principal varieties of Fuel used in the United States, and also in Europe, and on the ordinary Apparatus used for their Combustion. By Marcus Bull. Philadelphia and London, 1827.

The population of London and its immediate environs may be estimated at about two millions, and the annual consumption of coals within the same district does not fall far short of two millions of chaldrons, or seventy-two millions of bushels. Of this prodigious quantity of inflammable matter, a very considerable portion escapes combustion, and lodges in the form of soot in our chimneys, or is vomited forth to contaminate and cloud the atmosphere of the metropolis: so great is this loss, that independent of the mere advantage of getting rid of smoke, its prevention is an important economical problem; and though the rage for smoke-burning has passed over, we are quite certain that the subject still deserves the most serious attention, being convinced that, of the fuel consumed in the ordinary processes of warming our houses and cooking food, at least one-third is uselessly thrown away, and might be saved by a more economical and scientific construction of common grates and fire-places. All useful and well-conducted experiments, therefore, in

relation to these matters, deserve notice; and though much of Mr. Bull's essay is not applicable to our case, it contains a variety of interesting facts and information: his experiments appear to have been very carefully conducted, and should be consulted by all those who are engaged in similar investigations.

Meteorological Essays and Observations. By J. Frederic Daniell, Esq., F.R.S. Second Part, 1827.

We hope to be able in our next Number to enter into a detailed examination of the subject of Mr. Daniell's inquiries; at present, therefore, our object is merely to announce the second edition of his valuable and laborious essays, and the publication of the present *second part*, in which, for the convenience of those who possess the former edition, all the new matter is collected. It includes the following essays:—

1. On the Trade winds, considered with reference to Mr. Daniell's theory of the constitution of the atmosphere; in a letter from Capt. Basil Hall, R.N., F.R.S.

2. On evaporation as connected with atmospheric phenomena.

3. On climate, considered with regard to horticulture.

4. On the oscillations of the barometer.

5. On the gradual deterioration of barometers, and the means of preventing the same.

6. Addenda and notes—among which will be found a valuable table of the elastic force of aqueous vapour, calculated by Mr. Galbraith from the experiments of Dr. Ure, by the formula of Mr. Ivory.

Philosophical Transactions of the Royal Society of London, for the year 1827. Part II.

The following are the contents of this Part of the Society's Transactions:—

On a new form of the differential thermometer, with some of its applications. By William Ritchie, A.M., rector of Tain Academy. Communicated by J. F. W. Herschel, Esq., Sec. R.S.

On the structure and use of the submaxillary odoriferous gland in the genus Crocodilus. By Thomas Bell, Esq., F.L. and G.S.S. Communicated by Sir Everard Home, Bart., V.P.R.S.

On the permeability of transparent screens of extreme tenuity of radiant heat. By William Ritchie, A.M., rector of Tain Academy. Communicated by J. F. W. Herschel, Esq., Sec. R.S.

On the derangement of certain transit instruments by the effects of temperature. By Robert Woodhouse, A.M., F.R.S., &c.

On some of the compounds of chromium. By Thomas Thomson, M.D., F.R.S. L. and E., Professor of Chemistry, Glasgow.

Rules and principles for determining the dispersive ratio of glass; and for computing the radii of curvature for achromatic object-glasses, submitted to the test of experiment. By Peter Barlow, Esq., F.R.S., Mem. Imp. Ac. Petrop, &c.

On the change in the plumage of some hen-pheasants. By William Yarrell, Esq., F.L.S. Communicated by William Morgan, Esq., F.R.S.

On the secondary deflections produced in a magnetised needle by an iron-shell, in consequence of an unequal distribution of magnetism in its two branches. First noticed by Captain J. P. Wilson, of the Honourable East India Company's ship Hythe. By Peter Barlow, Esq., F.R.S., Mem. Imp. Sc. Petrop.

On the difference of meridians of the royal observatories of Greenwich and Paris. By Thomas Henderson, Esq. Communicated by J. F. W. Herschel, Esq., Sec. R.S.

Some observations on the effects of dividing the nerves of the lungs, and subjecting the latter to the influence of voltaic electricity. By A. P. W. Philip, M.D., F.R.S. L. and E.

On the effects produced upon the air-cells of the lungs when the pulmonary circulation is too much increased. By Sir Everard Home, Bart., V.P.R.S.

Theory of the diurnal variation of the magnetic-needle, illustrated by experiments. By S. H. Christie, Esq., M.A., F.R.S.

On the ultimate composition of simple alimentary substances; with some preliminary remarks on the analysis of organized bodies in general. By William Prout, M.D., F.R.S.

A Practical Treatise on the use of the Blowpipe in chemical and mineral analysis; including a systematic arrangement of simple minerals, adapted to aid the student in his progress in mineralogy, by facilitating the discovery of the names of species. By John Griffin, Author of Chemical Recreations. Glasgow, 1827.

Performing with the blowpipe is something like playing upon the fiddle—it looks mighty easy, but for its perfect accomplishment requires a combination of skill and dexterity which practice alone can confer. We are disposed, therefore, to think lightly of those essays upon the subject which pre- [illegible] instruct the beginner in the actual use of the instru- [illegible] telling him how he is to puff out his cheeks, breathe [illegible] nose, make a valve of his tongue, and keep up a

perpetual stream through the nozzle of the tube; all which is much easier described than done, and is entirely matter of experimental acquisition, more easily attained without than with the usual instructions. In the little work before us, all these matters are passed over with fit brevity, and the attention of the student is chiefly directed to the appearances which different substances exhibit before the blowpipe, and by which minerals may be distinguished and classed. The history of these constitute the bulk of Mr. Griffin's duodecimo, being preceded only by a few remarks upon the different kinds of blowpipe, respecting which we have merely to observe that justice is not done to Mr. Newman, who first suggested what is here called "Dr. Clarke's blowpipe;" indeed elsewhere the author seems a little angry with Mr. Children for recommending Mr. Newman's apparatus. We observe, moreover, that no notice is taken of Mr. Newman's and several other papers on the blowpipe, which have appeared in the old series of this Journal; nor of Dr. Clarke's original Essay, published in the second volume of that work, from which, and sundry other symptoms, we conclude that Mr. Griffin is a pupil of Dr. Thomas Thomson. Be this as it may, we bear him no malice, and very conscientiously recommend his book to the mineralogical student, as a valuable and clear epitome of what relates to the behaviour of substances before the blowpipe.

Circle of the Seasons, and Perpetual Key to the Calendar and Almanack; to which is added the Circle of the Hours and History of the Days of the Week, being a compendious Illustration of the History, Antiquities, and Natural Phenomena of each Day in the Year. London, 1828. Small 8vo.

The title of this book may lead our readers to suspect it as an interloper among works on science; but it touches upon many points of scientific inquiry, and upon botany especially, and is compiled with so much evident labour and accuracy, as to merit recommendation. The saints and festivals of each day are recorded, by which we make [illegible] of many worthy persons and curious [illegible] also a brief natural history of each day, [illegible] notices of the plants which on an average [illegible] to fade, and of the birds which arrive or [illegible]

The merits of the descriptive poetry, which is thickly interspersed, we leave to other critics. Those who are destined to live in the "fuliginous tenebrosity" of this smoke-saturated metropolis, and to breathe an atmosphere "sated with exhalations rank and fell," care little about the first peeping forth of the modest snowdrop, or the early bursting of the golden crocus; but such as reside in the country will be glad to have their attention pleasingly directed to the successive products of the field, the flower-garden, and the green-house.

Conversations on the Animal Economy. By a Physician. 2 vols. small 8vo. London, 1827.

We have more than once expressed our opinion on the subject of conveying information to young people in the way of "Conversations," which in the present volumes are carried on between Dr. A., Harriet, Sophia, and Charles; they are at once instructive and amusing, and evidently the produce of one possessed of much information upon the subjects discussed, and, what is more to the point, of the art of pleasantly and intelligibly conveying it.

The Conversations open with an account of the coverings or integuments of animals; their arrangements by systematic writers are then adverted to, and a short but useful description is given of the varieties of mankind, as enumerated by Blumenbach and illustrated by Camper. The bones and muscles form the subjects of the fifth and sixth conversations; they are concisely described, and with sufficient accuracy. The brain and nervous system and the organs of sense are next talked about. The doctrines of phrenology are fairly explained; and in the conversations on smell and taste, vision, hearing, and touch, the anatomy of the respective organs, and their varieties in the different animal tribes are treated of, the dulness of the details being relieved by physiological illustrations. The remaining conversations are occupied with an account of the principal functions of anim[illegible] several organs chiefly concerned in their perf[illegible] varieties of teeth and stomachs are here [illegible]ctures of the heart and blood-vessels, [illegible]culation and respiration. The produc[illegible]al systems is then noticed; and the [illegible]ding conversation is employed in the [illegible] general phenomena of growth and decay.

We have thus briefly stated the contents of these volumes, which are further illustrated by numerous woodcuts and several plates; and are perfectly ready to commend the performance as an extremely useful and proper book for young persons, but *not*, in our opinion, of both sexes: we should have been better pleased if Harriet and Sophy had been replaced by William and Thomas; for we cannot fancy the subjects here discussed as quite fit for young ladies. Boys, on the contrary, ought to know much more of these matters than they commonly do; and for conveying such information in a pleasing and familiar, yet neither vulgar nor superficial style, this compilation seems perfectly appropriate, and will, we trust, find, as it ought, a numerous class of readers.

Notice of a New Genus of Plants discovered in the Rocky Mountains of North America by Mr. David Douglas. By John Lindley, Esq., F. L. S., &c. &c.

Upon his journey across the rocky mountains in April 1827, in latitude 52° N., longitude 118° W., at an estimated elevation of 12,000 feet above the level of the sea, the attention of Mr. Douglas was attracted by a brilliant purple patch amidst the surrounding snow. On approaching it, he was surprised to find that the colour which had arrested his eye was caused by the blossoms of a little plant, from which the superincumbent snow had not yet melted away. The well-known Saxifraga oppositifolia immediately occurred to his recollection, and he at first imagined he had either discovered that species, or one nearly allied to it; but upon a closer inspection, he perceived that it was no Saxifraga, but a genus apparently new. Specimens having been submitted to me for examination since Mr. Douglas's return, the following description has been drawn up:—

The plant forms a thick tuft consisting of numerous perennial branched stems, the lower of which are [illegible] persis-tent decayed leaves and fruit of previous [illegible] are round, bright purplish brown, covered with [illegible] branched, short hairs, and densely clothed [illegible] leaves. The leaves are a dull glaucous green, [illegible]

linear, obtuse, about five lines long and three-quarters of a line broad, so closely covered with hairs like those of the stem, that the whole epidermis is hidden. Their veins are concealed by the hairs; but if the latter are removed, they appear to consist of a thickened mid-rib, and a few nearly simple spreading venæ primariæ. *The flowers* proceed from the axillæ of the upper leaves, from three to six on each little branch; at first they are sessile, but their foot-stalks subsequently lengthen by degrees until the fruit is ripe, when they are from three-quarters of an inch to one inch in length, and covered with the same sort of hairs as the leaves and stem. The *calyx* is hairy in like manner, obconical, angular, with five equal erect narrowly triangular teeth, about the length of the tube. The *corolla* is of a vivid purple colour, infundibuliform, wholly destitute of pubescence; the *tube* is a little ventricose and rather longer than the calyx, its whole length being about three lines; the *limb* is spreading, five-parted with cuneate, oblong, obtuse, segments; the orifice is guarded by five transversely linear calli, placed under each sinus, and corresponding to the same number of external depressions of the neck of the tube. The *anthers* are linear oblong, nearly sessile, opposite the segments of the corolla, and a little enclosed within the tube. The *ovarium* is superior, of an obovate figure, one-celled, with a central, free, fungilliform placenta, the lower edge of which has five teeth corresponding to an equal number of peltate ovula; the *style* is filiform, as long as the tube of the corolla, and continuous with the ovarium; *stigma*, a minute depressed cup. The *capsule* is of a cartilaginous texture, surrounded by the persistent calyx; one-celled, with five recurving valves; the *seeds* are two, peltate, oblong, convex on the outside, concave in the inside, dark brown, covered closely with minute dots or depressions; four only having been found, their internal organization has not been determined.

[illegible] us that, with the exception of the interior of [illegible] whole structure of the plant is determinable: [illegible] obvious that it is referable to Primulaceæ, of which it [illegible] all the characters. In fact it is closely akin both to Primula and Androsace. From both these genera, however,

its ovarium which exhibits the greatest instance of reduction of ovula yet known in the order, and its dispermous capsule, with oblong concave seeds, readily and essentially distinguish it.

I have, therefore, named it after its indefatigable discoverer, whose active and successful researches in its native country, richly entitle him to the distinction.

DOUGLASIA.

Nat. Ord. *Primulaceæ; inter* Primulam *et* Androsacen.

Calyx obconicus, angulatus, 5-dentatus. *Corolla* infundibularis, tubo ventricoso, limbo plano 5-partito, fauce callo lineari sub utroque sinu. *Ovarium* uniloculare placentâ centrali liberâ pedicellatâ fungilliformi, margine 5-dentato; ovula 5 dentibus placentæ opposita. *Capsula* vestita, unilocularis, 5-valvis. *Semina* duo concava scrobiculata.—Cæspes *suffruticulosus* (Americæ borealis), foliis *indivisis,* pube *rigidâ ramosâ,* floribus *axillaribus solitariis.*

Sp. 1. *Douglasia nivalis.*

A Description of the Aurora Borealis seen in London on the Evening and Night of the 25th of September, 1827; with Critical Remarks upon other Descriptions of the same, and previous Appearances of the Meteor, both in the Northern and Southern Hemispheres. By E. A. Kendall, Esq., F.S.A.

On the evening and night of the 25th of September last, the horizon of the metropolis, toward the north, and toward the north-west and the north-east, exhibited a remarkable display of the meteor or phenomenon called, after the example of the Italian philosopher Gassendi, Aurora Borealis.

The weather, for many days preceding, had been mild, with alternate sunshine, clouds, and showers. The wind had been generally in the west and south-west quarters; though on the 18th and 19th it was in the north-west, and on the 20th in the north-east. The barometer, at three o'clock in the afternoon, had stood at from 30° 40′ to 30° 20′, to which latter height it had descended on the 20th; and, from that day to the 25th, it had remained at 29° 9[illegible]. The thermometer, at the same hour, between [illegible] the 20th, had ranged between 65° 6′ and 59° 2′; [illegible] on the 25th, at 59° 6′, with the wind in the sou[illegible] The sky, toward the zenith, on the evening of that day, [illegible]

tially clear, and partially covered with shifting clouds. On the north, and on the west and east of north, heavy and stationary clouds blackened the whole horizon, to an elevation of more than five degrees; and the southern hemisphere was dark with dark clouds from the horizon to the zenith.

I. By some, the Aurora was seen from the time when the sun was set; but the first appearance in the heavens, which attracted the attention of the present writer, whose situation at the moment shut out from him the horizon upon all sides but the west, was that of a certain breadth of red or copper-coloured light, or of light of a colour nearly resembling that reflected by an ordinary conflagration of buildings, pointing upward from the west. The colour, indeed, was dissimilar from that which is usual upon the occurrence of a fire on a cloudy night; yet, in the absence of any other immediate explanation, he should not have hesitated so to understand it, except for the figure within which it was circumscribed, and which, instead of being diffusive, and less and less conspicuous toward its extremities, or rounded in its outline, like masses of ruddy smoke, had the peculiarities of an equal breadth, rectilinear sides, a square top, and sharp outlines. Its height was continually increasing; but not even that phenomenon, nor even the curve to the eastward, across the heavens, and which it presently began to add to its figure, were appearances absolutely to dissipate the illusion of the existence of a fire; and it was scarcely, therefore, till this breadth of colour, throwing itself entirely over the heavens, and descending, at its projected extremity, toward the east, formed an arch, of which, perhaps, the elevation was seventy degrees, (which was not the work of many minutes, the motion, at the same time, being visible, but of moderate rapidity,) that its real character of a natural phenomenon distinctly impressed itself upon the mind of the present writer, its observer. While this, however, was proceeding, the road which he was pursuing had brought him more into view of the north-western and northern horizon; and, then, the light in the north, and to the west of north, which, from behind the clouds that lined the horizon, seemed like the light of a rising moon, or of the

breaking day, together with the vertical projection of rays of light, beneath and above the arch, removed every doubt as to the cause of the appearance, by demonstrating its connexion with an Aurora Borealis.

It was now about a quarter past eleven o'clock. The sky, beneath the lower or inner edge of the arch, was clear and star-light, and, through the contrast created by the ruddy colour placed against it, appeared of a lively blue. The upper edge of the arch, in the meantime, was relieved only by the dark gray of the clouds, which, with more or less continuity, overhung the upper part of the heavens. But these latter were now dispersing; the cloudless zenith, which presently afterwards disclosed itself, was now progressively and swiftly preparing; and, as the clouds moved and fled, the outlines of the arch lost their sharpness, the colour changed, from that of fire or of copper, to something more or less of purple or of the rose; it spread itself in the vapour, and with the vapour vanished.

II. But this was only the curtain of the stage, behind the folds of which the true scene had its existence. This latter, still concealed, to a certain and uniform height, by a parapet, as it were, of dark and unbroken clouds, consisted, first, in the ground of white light, already described as resembling that of a sky in the midst of which clouds shut out the disk of the moon, or rather that in which the rising sun is just about to appear; and, secondly, in a range of columns, or fountains, or jets of light, more coloured than the ground, which, rising from behind the ridge or parapet of clouds, and from and in the midst of the white light, formed, together, not the figure which would have been produced by their uniform convergence toward the zenith, but one which bore some resemblance to that assumed by the sticks of a fan, or still more to the appearance of stalks in a flower-basket, or in a sheaf of corn. For, in this manner, the column, which, in general terms, may be called the central one, and which arose in the due north, was vertical, and therefore projected toward the zenith; while those which extended from it upon either side, that is, toward the west or toward the east, gradually inclined more and more

toward the horizon on their respective sides; and, as to the outer columns on the east, inclined, not in rectilinear figures, but in curves more or less decided. In these columns or coruscations several particulars were to be remarked.

1. That, within the space of from one hour to two, the whole group appeared to traverse the horizon together, from the west of north to the east of north, as if upon one movable base, or as if the source of their appearance became gradually exhausted to the west of north, and grew gradually into activity upon the east of north; alternatives of explanation, however, which might materially affect the theory of their production. During the whole change, in the meantime, the north preserved its splendour, appearing uniformly as the focus of the fire, or as the pivot of the machine, or as the well from which all else was supplied. The change consisted in the appearance of columns, of more or less magnitude, strength, and brightness, more or less advanced from the north toward the west, or from the north toward the east; but the north, during all this variation, suffered no other change than this, that whereas, in the beginning of the evening, the greater portion of the columns rose to its west, while, in the latter part of the night, the greater portion arose to its east. But, besides this general configuration, and this united motion of the meteor, there was to be observed, in the several columns themselves, both the variations of colour which distinguished one from another, and the irregular and independent movement of each, always in the direction of its length or altitude, and situate in the interior, as it were, of its body; and also that peculiarity of form which distinguishes these coruscations from all other luminous appearances.

2. The colours of the columns, in that part of their height which is nearest their base, and where, as a ground, they had only the white light of the horizon, by which, and by their motion, and it should, perhaps, be added, by their vividness, they were distinguished, is a point upon which the writer speaks with some hesitation, and with respect to the more close observation of which he could like to enjoy a second opportunity of beholding the phenomenon. The variety and [illegible], and sometimes the terrible grandeur, of the colours

exhibited in the Aurora Borealis, is the constant theme of spectators and naturalists; and, upon the late occasion, an observer, apparently of more regularly scientific habits of pursuit than himself*, has particularly insisted upon a column, of a violet colour, rising west of north, and the place of which he thinks corresponding with that of the *magnetic pole*; a coincidence from which, as it may seem, he would believe a confirmation of the *magnetic theory* of the production of the Aurora to be obtained. In setting down the present description, the writer tasks himself to the most faithful description of what he actually saw, and suppression of all desire to support or condemn a theory, of which his mind is capable; and by those rules, therefore, the whole statement will be guided. His description already differs from that of some of his fellow-witnesses, as will be expressly considered below; but he confesses that while, in point of persuasion, he much inclines to the idea, that all the light displayed by the Aurora is in itself white, and only tinctured to the eye of the spectator by the atmospherical medium through which it is seen; and while, with respect to all those deeper colours, whether crimson or purple, or blood-colour, which appal the superstitious, and are described by the picturesque narrator as exhibiting the terrible in matters of vision, he judges it supposable that the whole machinery consists in the same interposition of vapour, near the horizon, which so often gives to the sun and moon themselves the appearance of being coloured like blood: while, therefore, he still adheres to his opinion, that the colours ascribed to the Aurora are wholly extrinsic; and, to borrow the words of a scientific writer, "dependent upon the medium through which they are seen;" he is obliged to acknowledge, that it did appear to him, that the several columns, in truth, were yet variously coloured, of pale, but bright and pleasing colours, from a pale yellow to a pale pink and a pale violet, and this in the direction of their height or length,—a phenomenon which wholly excludes, as to those columns and their colours, the influence of an interposing medium, the effect of which would be perceived horizontally, and across the whole range of columns, or part of the range, and not ver-

* Literary Gazette, Sept. 29, 1827.

tically nor obliquely, according to the direction of each column, and within the limits of its sides. He confesses, also, that he did take notice of the pale, but bright violet-coloured column, distinguished also by its breadth and height, and situated to the west of north; but which column, he is surely right in adding, ultimately moved, with those next to it, toward the north. He distinctly and pointedly observed, at the same time, that the columns which stood due north were always white, and that the colours of the other columns appeared to strengthen in proportion as they were distant from the due north, either west or east; and he came to a fixed conclusion, while the phenomenon was under his eye, that, to his judgment at least, the strength of the fire, so to say, was in that point of the horizon which lay due north; and that there was a diminished brightness, with a proportionable increase of colour, to the right and left.

3. As to the separate movements of the columns, these, in the first place, were quick, and forced upon the eye, while the movement which gradually deployed or advanced the right wing of the celestial arm, and gradually contracted or withdrew the left, was slow, and perceived only by its results; and, in the second place, while these latter were parallel to the horizon, the former were either vertical, or in the oblique or curved direction of the bodies of the columns. But this motion consisted either in vibration, or in irregular but alternate projections and contractions; and the motion of each column, as has been said, was independent on that of others. Rarely two adjoining columns were in motion at the same time. Almost always the moving column or columns were seen to start from the midst of others, which, for the time, were quiescent, but which had had their turn before, and would presently have it again. What eminently struck the writer, however, was the internal motion of that to which he cannot allow himself to give another name than that of the apparent luminous material of the columns. It seemed to him as if the volume of each column or coruscation was itself composed of parallel lines of luminous matter, arranged in the direction of the column, and every one of which was separately the subject of movements similar to those of the entire

column, or entire bundle of lines; or as if the whole column were like the stalk of a plant, and filled with upright and luminous fibres, or like a skein of thread, drawn vertically or obliquely, and of which each particular thread should have particular motion in the direction of the whole; or (what he thought the comparison which proclaimed the very nature of the material of the columns) like fountains, or jets of water in the sun, in which every particular particle should be moving in the general direction of the jet, and yet each moving and shining for itself.

4. And this apparent nature of the substance of the columns or coruscations allies itself to what finally regards them; namely, their form. In this description, they have hitherto been spoken of by the name of columns or pillars; and the similitude, which that name suggests, is justified by the general figure of all the lower parts of their bodies, which, unlike the figure of rays of light on the one hand, and unlike that of flames of fire on the other, is a tall or lengthened object, of small comparative diameter or breadth, and of which the sides consist in right and nearly parallel lines. But, by the English, these columns, pillars, or coruscations, were anciently called so many *burning spears*; and they have also received the names of *streamers* and *pencills* *, which two latter, in the history of appurtenants of war, signify long and narrow, and pointed banners or flags. Their similitude to flags is excusably fancied from their quick, capricious, and irregular motions; but their likening to "spears," is that which may claim to be thought the most felicitous, as to the true conception of their form, as it is also that, the idea of which contributes to render the phenomenon the most fearful in the

* "PENCELLS.—Pencills, or flagges for horsemen, must be a yard and a halfe long." Harleian MSS., cited in an interesting and valuable essay on the "Banners used in the English army, from the Conquest to the reign of Henry VIII." By N. H. NICOLAS, Esq., F.S A.—*Retrospective Review*, Oct. 1, 1827.

"The Pensell, or Pennoncelle, was the diminutive [illegible], being a long narrow flag."—MEYRICK'S ANCIENT ARMOUR.

"STREAMER.—A Streamer shall stand in the toppe [illegible], or in the forecastle, and therein be put no armes, but a man's conceit or [illegible], and may be of the lengthe of twenty, forty, or sixty yards; and it is slit as well as a guydhomme or standarde."—HARLEIAN MSS.

An item, in a bill of parcels, charged to the Earl of Warwick, in 1437, consists of "a great Stremour for the Ship, of xl yerdis lenghth, and viij yerdis in brede."—BANNERS USED IN THE ENGLISH ARMY, &c.

survey of ignorance and superstition. But the SPEAR-SHAPE is descriptive, because the coruscations, unlike rays of light, and unlike flames of fire, have neither the obtuse figure of a pyramid, nor the acute one of an obelisk, upright or reversed; but, after rising, through almost their whole height or length, of an equal or nearly equal diameter, terminate in a point which is formed, not of right lines, like the point of a dagger, but of curved lines, so as to form the rounded point of a spear, or that figure which is so familiar to botanists, as spoken of "spear-shaped" leaves. A ray of light, in whatever direction it is thrown, broadens, with right-lined sides, from the first point of its departure, to the furthest stretch of its projection; a flame of fire points uniformly upward, with the same regularity of form, excepting only as it is liable to undulation from the motion of the atmosphere; but, the columns, spears, streamers, or coruscations of the Aurora Borealis, have no form but that under review.

5. About half-past eleven o'clock, or nearer to twelve, several powerful columns shot toward the zenith; while, to the east of north, others were at once curved in their form, and projected in an angle of about thirty degrees with the horizon. But while, upon the west of north, the sky, above the ridge of clouds, was entirely clear, so that, there, the columns played upon a ground which formed a slight contrast with themselves, here, the clouds were still heavy, and the columns behind them appeared, in consequence, of a fiery red, deepening as they approached the outer edge of the whole display, at which was the sharpest outline, contrasted in the distinctest manner with the dark sky. The light upon that side called to the mind of the writer the "dunnest smoke of hell," of Macbeth; while, as to its outer line, as seen from the east end of Pall-mall, the sides of the stone spire of St. Martin's church, which rose to the eastward of it in the sky, were not more [illegible] defined; the dark intervening sky affording relief [illegible]ugh not equally so, upon account of the [illegible]ness of even the obscured columns. But, in [illegible] leave of this columnar, or spear-like, and main part of the Aurora, it may be permitted to add, that, in those tapering forms, together with their motions, (though the comparison

may still be sufficiently remote and fanciful,) it was easy to discern the origin of their having been resembled to weapons of war; that is, to the spears of an army, raised, lowered, laid at angles, and gleaming, glittering, crossing, and clashing in battle. And equally, too, from their quick, varied, and separate, and, as it were, whimsical motions, might they reasonably receive, in their milder displays, and in moments of more peaceful and cheerful association, the very different name of *merry dancers!*

III. Though, as will presently be found, it is the ruling idea of the present writer, that the Aurora Borealis is a single object, its appearance, when unmodified by the accompaniments of clouds or fogs, being merely that of its own coruscations, playing in the free expanse, yet for the purposes of analytical description and contemplation, it is here thought convenient to divide it into the three parts in which, through the temporary and accidental intervention of the coloured arch before-mentioned, it appeared in the night now in recollection. These three supposititious parts, then, may be understood as follows: first, the arch, belt, or band which was temporarily thrown across the heavens; second, the main body of the coruscations below the arch; and third, the coruscations above it, and in or near the zenith. It is of these only that it remains to speak.

It was not till about midnight that the zenith itself (which, however, formed the southern boundary to this part of the display) became the scene of a class of appearances, differing, indeed, essentially, in their forms, from those in the horizon, but closely connected, as it may be believed, with all the materials, and all the movements, of these latter. The zenith, at that hour, was cloudless, and resplendent with stars, and the air was freshened by a gentle breeze from the south. Between the earth and the stars above, ther[illegible]arent intervening vapour, and nothing, therefo[illegible]o-spherical fluid which eludes the sight. But, [illegible] medium, if such only it was, coruscations were [illegible] continually shooting, of which the appearance was, that it overspread this portion of the vault of heaven with an ever-shaken

sheet of thin, gauzy, white, or yellowish-white, and nebulous, or cloudy matter. To the writer, this superior portion of the Aurora, though not the most lustrous, and, therefore, not the most striking of the whole, was yet by no means the least interesting and inviting to attention; for, here, as it appeared to him, the *material* and the manner of operation of the meteor were brought nearer to the eye, and exhibited with such a back-ground (the starry heavens) as gave a transparent view of the same matter as that, which, (as he thought,) seen vertically, and in the horizon, appeared comparatively, at least, opaque. The transparent medium, however, above, through which, even when shook or vibrating, and even when whitened with light, the stars were always seen in more or less brightness, was now in continual motion; or, meteoric light or matter was continually, though irregularly, and as it were, playfully shot through it. The illuminated substance (whether the atmospherical fluid, reflecting the light of the meteor, or the luminous body of the meteor itself, but probably the latter) was incessantly discovering itself in different places; now here, now there, now bright, now dim; but far less in a manner, or with an appearance, to be compared with lightning, than with such as resembled the changes of ripple upon the bosom of a wide-spread water, when a variable breeze blows over it; first in one part, and then in another; and now in one direction, and the next moment in a second. Or, the canopy of heaven, at this time, might be said to be composed of a lace or gauze bearing a figured pattern, of which the fluttering motion continually changed the places, or hid or re-displayed the figures represented; or the picture, perhaps, will be more easily imagined, if conveyed in the very appropriate language of an older hand, which, referring to the appearances displayed in the zenith, remarks, "They break out in places where none were seen before, skimming briskly along the hea[illegible]enly extinguished, and leave behind a uniform [illegible]: This, again, is brilliantly illuminated in [illegible]nner. and as suddenly left a dull blank." It [illegible] understood, however, that, at least as seen by the present writer, in this mixture of white and blue, the blue was always the preponderating colour; or, in other words,

that, the field of the unoccupied zenith always bore a large proportion to the space or spaces covered, however momentarily, with light, or with the luminous substance. For the rest, the particular mentioned in the passage which has now been quoted, namely, that of the residue of a dusky track, after the departure of the white light, did not, if it was there, attract the attention of the present writer, upon the late occasion; but he certainly, in many instances, remarked the return of the light to the places in which it had been visible before; and this feature, either with or without that of the continuance of a dusky track, is possibly capable of adding some support to the general opinion which he conceived at the moment, which all subsequent information has still allowed him to retain, and of which he proposes to make further use; namely, that the appearances in the zenith are only extended exhibitions of the luminous phenomena in the horizon, or their southern extremities, or the tops of columns projected from the northward. He thought that, in the zenith, he saw the same material, parcelled out, attenuated or diluted, spread thin, and, as it were, shown with greater transparency, with that which, in thicker volume, with more accumulated strength, intenser light, with more solid body, and withal behind a denser mass of asmospherical vapour, rose, and glowed, and sometimes gloomed, in the horizon. But, be this as it may, it is, perhaps, this upper part of the exhibition, in which the lights or streamers seem to interweave, or cross and recross each other, to dance in and out of the area, and to indulge in motions still more capricious or anomalous than is probably the real fact; it is, perhaps, this upper part which has alternated, as before recalled to view, the names and similitudes of spears, gleaming, glittering, interposing and clashing as in battle, and of *merry dancers*, the latter the gayer comparison of the dancing north.

IV. The Aurora continued to fix the attention of the writer till between twelve and one o'clock of the morning of the 26th; and he presumes that it continued visible till the superior light of the rising day eclipsed its glory. The 26th was warm, but oppressed with fog, through which the sun broke

only at intervals; and, between four and five o'clock in the evening, a small but steady rain commenced, and continued, or rather increased in heaviness, till after midnight. Between eleven and twelve, while it still rained, the writer, on looking at the sky, which was covered with a uniform mass of clouds, the writer observed, from point to point, over the northern and southern hemispheres, a glow of ruddy light, which he suspected, and still suspects, to have been produced by the light of the continued Aurora, reflected by the vapour. He took the opinion of a fellow-traveller, which coincided with his own; but it has not come to his knowledge that any individual, himself and his companion excepted, has formed a similar conjecture—nor, indeed, is it impossible that it was no more than the light of the hidden moon. The night of the 27th was starlight, though with fog near the surface; and there was then no appearance of an Aurora. The night of the 28th was remarkably clear, and there was still no return of the Aurora. The morning of the 29th was warm, with continued and heavy rain; but, after this, there succeeded a week or more of clear and dry weather; and these united particulars close the history of the phenomenon, as far as belong to the personal observation of the writer. The direction of the winds, and the state of the barometer and thermometer, were of the same general description, during many days subsequent to the appearance of the Aurora on the 25th, as that which had belonged to them from the 20th, and almost for many days before, and of which the particulars have been stated above; and these remarks may merit record, as connected with the question of the ordinary duration of the Aurora, and of the weather by which it may be thought produced, or which it may be thought to bring. In many instances, it has been observed, even in its splendour, and even in southern latitudes, for several nights in succession; and an in-[illegible] weather has likewise been expected from its [illegible] Upon this occasion, there was no remarkable [illegible] latter till the night of the fourteenth day after [illegible] (October 10th), when there occurred a violent gale of wind from the south-west, accompanied with loud thunder, and the most vivid lightning; subsequently to which, as usual,

the air, for a few days, was felt to be cooler than before. It has been said, that a gale of wind, from the south-west, is always to be looked for within twenty-four hours after the Aurora.

V. The astronomical writer, already more than once mentioned, speaking of the Aurora of the 25th of September, describes it as "that mysterious phenomenon;" and Mr. Adams, the meteorological correspondent of the publication referred to, records it as, "perhaps, as conspicuous as any that has ever been seen in England*;" so that, assuming these impressions in both instances to be well founded, neither the present state of science upon the one hand nor the specimen of the phenomenon upon the other, are such as to discourage either of the objects of the remainder of these pages; namely, the one to contribute, as fully as possible, to the completion of a faithful account of the Aurora, as seen in London upon the late occasion, by uniting, and by analysing the descriptions that have caught already the eye of the writer; and the other, to correct, and to enlarge if it should be practicable, the natural history of this description of meteor, by the comparison of what has hitherto been usually written upon the subject, either descriptively or philosophically, as well with the results of the late actual observations, as with the several facts or opinions more anciently registered. According to some, the interval which had elapsed, since an equal or a superior display of the phenomenon was witnessed in London, is twenty-four years, and, according to others, thirty-six: nor is the scanty list of examples scientifically recorded, at all inconsistent, from the wide separation, as well as irregularity of its dates, with such a view of the infrequency and uncertainty of any considerable appearance in other southern latitudes. The opportunity, therefore, now offered, ought not, perhaps, to be neglected; and the writer is not wholly without the prospect, that, upon a re-examination, both of opinions and facts, some satisfactory inevitable conclusions may be elicited, both as to the history and the theory of the meteor, hitherto, the one hastily received,

* Meteorological Journal, Literary Gazette, Sept. 29th.

and the other negligently overlooked, or unwarrantably contradicted. The paragraphs, then, which immediately follow, will connect and review the accounts of the writer's fellow-observer of the 25th of September; while those which succeed will be devoted to a brief enumeration of statements already recorded in books; though, to a certain extent, both these paths will involve us in mixed investigations, historical and theoretical.

1. "It first appeared," says Mr. Adams, who dates from Edmonton, in Middlesex, "about eight o'clock in the evening, as a strong white light, much resembling the approach of sunrise; and so continued till a short time after eleven, when a considerable number of dark clouds collected toward the north and north-west, and several streaks of a pale white light were seen proceeding from the clouds, and reaching nearly to the zenith. But the most remarkable part of the phenomenon was exhibited in a N. N. E. direction, where, at about 30° above the horizon, was a small dense cloud, above which was a broad streak curved, and about 10° in length, varying in colour from a deep copper hue to a red." "From this," continues Mr. Adams, "the coruscations were incessant, and remarkably bright, darting frequently to the zenith, where they were frequently crossed by others equally bright and numerous, proceeding from the west toward the east."

2. The astronomical writer, who dates from Deptford, describes the phenomenon as commencing at a quarter past eight o'clock, and travelling, from west and north-west, to north-east; and the streaks, or streamers, or, as he denominates them, the flashes, "converging to the zenith," and "coruscating with great velocity." He also particularises the peculiar appearance of "a streak or column of a phosphorescent violet tinge;" and adds, "The *two* red beams of light, seen in the *easterly* and *westerly* direction [directions], were diametrically opposite to each other, and ninety degrees distant from the violet light (by far the most luminous, though comparatively quiescent), which was to the west of north, *and therefore could not be far from the magnetic meridian*, which would be crossed at right angles by a line joining the places of the red beams. The southern edges of these were accurately defined, not blending with the adjacent azure, but most distinct from it, and per-

pendicular to the horizon." Finally, this gentleman speaks of the general luminous aspect, as "much resembling the tail of a comet," and says, that Ursa Major, and other stars, were visible through its medium; that three meteoric stars also appeared, during the phenomenon, in the east and north-east; and that the entire horizon was obscured by dark, heavy clouds, from three to five degrees in height*."

3. Besides these observers, two or three others, if not many more, less scientific, perhaps, but yet entitled to attention, have communicated to different newspapers their accounts of the same phenomena. "The metropolis," says one of these, "was surprised on Tuesday night by a brilliant display of Northern Lights, which but very seldom stray so far south. The last which we beheld in London were in the autumn of 1801, about the end of September, or beginning of October; and the fancied prodigy filled all the superstitious heads, at the time, with fearful prognostics, and loosened the tongues of a hundred prophets. The spectacle, then, was truly magnificent. On Tuesday night (the 25th) the northern parts of the heavens displayed, about eleven o'clock, so ruddy a blaze, as to appear like the reflection of a mighty conflagration. An hour later, the red hue was gone; but the whole horizon, from the north to the east, was lined with *a thin cloud*, from which the rays of light rolled, or sudden rays flashed up, and as suddenly vanished, to appear in a different part." "At about half past eleven o'clock," says a second, "my attention was attracted to a singular appearance of light and streakiness in the sky. I observed it for nearly two hours. The sky, to the north, was obscured, for about fifteen degrees above the horizon, by a *dense* stratum of black clouds; from the upper edge of this, the light became first apparent, extending from nearly north-east to north-west, exceeding considerably in power that arising from the moon just previous to its rising. From this broad stratum of pale yellowish light shot beautiful *pencils*, of a luminous, hazy appearance, up to the very zenith, changing momentarily in length and intensity. During this period, the wind blew gently from the south; and I frequently observed, that when it fresh-

* Literary Gazette, as above.

ened a little, the Aurora Borealis became more brilliant in its appearance, sending beautiful coruscations of light, in rapid succession, towards the zenith, and frequently passing that point ten or fifteen degrees to the southward. I have been assured, by those who are well acquainted with this beautiful phenomenon, that they have not seen any appearance of it equal in brilliancy and beauty to this, for upwards of six and thirty years." "Last night," says a third, "we were favoured with that interesting phenomenon, the Aurora Borealis, or Northern Twilight, which so often amuses and cheers our neighbours in the north, but seldom, I believe, is seen in our latitude. It was without those varied colours," adds this writer, "which cause it to be a grand spectacle in those regions." "Not far from the horizon," he adds, "in the northern hemisphere, were transparent bodies of light, *eclipsing* the brightness of the stars, which, however, were perceptible through it. From hence, beams of light, varying in degrees of brightness and breadth, shot up towards the zenith; here streamers of light flew from the east to the west, and from west to east. The southern hemisphere was cloudless, the stars shining with brilliancy. By the light of this phenomenon, I could discern the time of night, which was between eleven and twelve, as well as other objects, as they appear on a moon-light night, when the moon is obscured by clouds." "The sky in the north," we are told by the fourth, "appeared as if a light shone from behind some dark masses of clouds. As I approached Hampstead, the silvery light was gradually tinged with rosy spiral streams, like those which sometimes precede the rising and follow the setting sun. These spiral red streaks did not appear to move quickly; but they were subsequently followed by the *merry dancers*, which fully maintained the character bestowed upon them by our northern neighbours. After passing through Hampstead, I crossed the heath, and came down what is called North-end Hill, to Golder's Green, Hendon. When you arrive at the foot of the hill, you enter upon the open part of Golder's Green, where you have a clear and unobstructed view of the sky from west to north. I never shall forget the grandeur of the scene which awaited me there. A continuous border of dark cloud skirted the horizon completely from west to north, whilst

from behind it, incessantly and rapidly shot up the most beautiful coruscations of white light, which, being relieved by the dark border, added double brilliancy to the ever-shifting scene."

VI. But, after transcribing these respective accounts, it may be permitted, for the purpose of uniting them with that submitted in the preceding pages, to remark,

1. That the account by Mr. Adams, of the appearance worn by the Aurora at an early hour in the evening, is, no doubt, entirely correct; and that it is easy to understand, from this description of that early appearance, why little observation was attracted to the phenomenon till about eleven o'clock at night, the time assigned, as well in this, as in all the other accounts, for the commencement of the phenomenon.

2. That the "streaks of a pale white light," which Mr. Adams describes as proceeding, a short time after eleven, "*from* the clouds," must be understood, as stated by the writer last quoted, as proceeding "*from behind* the clouds;" that, when the astronomical writer at Deptford speaks of Ursa Major and other stars being seen through the Aurora, it must be recollected, that, perhaps, this remark should apply to the medium of the thin and shifting lights in or near the zenith; and,

3. That it is with respect to the "broad streak, curved," of Mr. Adams; the "*two* red beams of light," of the astronomical observer at Deptford; and the "arch" of the present description, that the principal, if not only discordance obtains. Neither of the other three writers appears to have seen any thing, whether of one "broad streak, curved," and "varying in colour from a deep copper hue to a red," or of "*two* red beams," as spoken by the writer at Deptford; while, in each of the three accounts in which that part of the phenomenon is actually referred to, the descriptions are materially dissimilar:—

1. The writer at Edmonton mentions only a single streak, while the writer at Deptford speaks of *two*.

2. The writer at Edmonton describes his single streak as curved, while the writer at Deptford says nothing of curvature; and, in describing the position of the beams as "perpendicular to the horizon," may seem to leave no curvature to be understood.

3. The writer at Edmonton seems to lift his "broad curved streak" much above the horizon; for he first places a small dense cloud 30° above the horizon, and, then, his broad streak above the cloud; thus describing a curve of which the situation was near the zenith, while the writer at Deptford is describing "two red beams," standing perpendicularly to the horizon.

4. The writer at Edmonton places his "broad streak, curved," "in a N. N. E. direction;" while the writer at Deptford records "two red beams of light, seen in the easterly and westerly direction." Lastly,

5. The writer at Edmonton seems to make coruscations, "incessant and remarkably bright," dart from his "broad streak, curved;" while the writer at Deptford seems only anxious to place his "two red beams," as perpendicular pillars, standing on either side of the *magnetic meridian.*

VII. And, from the whole of this, from the total silence of four accounts, and from the extreme discordance of the other three, the present writer presumes to draw the following inferences, including that of the accuracy of his own original statement:

1. That the two perpendicular red beams of light, of the writer at Deptford, should be joined with broad curved streaks of a deep copper, or red hue, of the writer at Edmonton, to complete the *arch* which has been spoken of in the foregoing pages.

2. That this *arch,* or curved streak, with its feet east and west, sent forth no coruscations itself; but that the coruscations rose beneath it, and passed above it.

3. That it was described upon the clouds only; was no part of the Aurora; and, from its connexion with the clouds only, had an evanescence which, on the one hand, was the cause of the various descriptions, and, on the other, of no descriptions at all. The present writer observed this part of the phenomenon from its beginning to its ending He saw it rise in the west, extend itself from the north, and descend in the east; and he thinks it reasonable to ascribe the variations concerning it, in the coincident narratives, to the different points of time to which alone they really refer. The writers at Edmonton

and Deptford seem to have had their attention fixed upon it at different epochs of its progress; and all the four other writers, who have been cited, seem to speak of a time subsequent to its disappearance. The present writer does not recollect the small cloud below it, spoken of by Mr. Adams; but he well remembers the clouds above it, and along and near the northern edge of which it seems to be formed. He does not recollect seeing its definite *southern* outline contrasted with the azure sky; but he well remembers seeing that outline contrasted with the dark clouds above it, or to its southward; and also the contrast of its definite *northern* outline, as contrasted with the azure sky beneath.

VIII. It is necessary to take notice, also, of what is said above, by the astronomical observer at Deptford, as to the "flashes converging to the zenith," and, further, of the omission, both by this writer and by Mr. Adams, to speak of the curved beam, streamer, or coruscation, to the east of north, as described above. The whole veracity of the foregoing description depends upon the denial of a uniform convergence of the streamers, pillars, columns, or coruscations toward the zenith; nor was it, in all probability, the intention of the writer at Deptford, to assert any such convergence, but only to speak of those coruscations, or shifting lights, in the zenith, which are described by Mr. Adams as crossing each other from east to west. It is remarkable, at the same time, that neither the one nor the other of these writers have mentioned that direct reverse of convergence which marked the general figure and arrangement of the streamers or columns of the Aurora, and which was so opposite to what would have been given to it by the phenomenon of convergence. Indeed, the violent curve of the extreme column to the N. E. or N. N. E., shrouded, too, as that column was with a body of dense vapour through which its light appeared of a deep and dull red colour, might make the description of this itself answer to the "broad streak, curved," of Mr. Adams, if we were not certain, from other particulars mentioned, that Mr. Adams really refers to the curve which formed part of the *arch*. For the rest, no mention of the real directions of the several columns having been made by any observer of the Aurora of the 25th

of September but himself, and especially none of the outward curve of the easternmost column, it is satisfactory to the writer to have found an account of an appearance similar to this last, in an Aurora of which he will presently have occasion to speak.

IX. Finally, there is an observation to be made upon that part of the description, by the second correspondent of the newspapers, where it is said, that during the appearance of the coruscations in the zenith, "the wind blew gently from the south," and the spectator "frequently observed, that when it freshened a little, the Aurora Borealis became more brilliant in its appearance;" to which it may also seem the writer's intention to add,—"sending beautiful coruscations of light, in rapid succession toward the zenith, and frequently passing that point, ten or fifteen degrees to the southward." Now the reality of any dependence of the light and motion of the Aurora upon the freshening of the breeze, would seem too strongly to affect the question of the nature and action of the *auroral matter*, to be admitted without cautious examination. In truth, what was it that constituted the luminous matter which we saw in the zenith? The stars were visible through it. But for luminous appearances that flew or skimmed along the heavens, we should have said, that the latter were clear, and that there was nothing but the purest atmosphere between the earth and the heavens. Was it, then, the atmospherical matter which was thus illuminated, and which, being ruffled by the breeze, can be supposed to have really exhibited the appearances described by this writer, or, was it not, rather, illuminated *auroral matter*, which was shot through the atmosphere; and, if this last, how are we to understand that its brilliance, and still less the frequency and vigour of its coruscations, could have been affected by the freshening of the breeze?

X. But, taking, now, a final leave of the description of the Aurora of the 25th of September, and of the observations specially suggested by it, let us here examine the several particulars which are commonly offered as part, at least, of its true history; an undertaking, for the greater convenience of which the account given in a modern work of much and

deserved reputation, shall be quoted and considered sentence by sentence, as follows:

1. "AURORA BOREALIS, *Northern Light*, or *Streamers*; a kind of meteor, appearing in the Northern part of the heavens, mostly in the winter time, and in frosty weather.

2. "It is in the Arctic regions that it appears in perfection, particularly during the solstice.

3. "In the Shetland Islands, the Merry Dancers, as they are called, are the constant attendants of clear evenings, and prove great reliefs amidst the gloom of the long winter nights.

4. "They commonly appear at twilight, near the horizon, of a dun colour, approaching to yellow; sometimes continuing in that state, for several hours, without any sensible motion, after which they break out into streams of stronger light, spreading into columns, and altering slowly into ten thousand different shapes, varying their colours from all the tints of yellow to the obscurest russet.

5. "They often cover the whole hemisphere, and then make the most brilliant appearance.

6. "Their motions, at these times, are most amazingly quick, and they astonish the spectators with the rapid change of their form.

7. "They break out in places where none were seen before, skimming briskly along the heavens; are suddenly extinguished, and leave behind a uniform dusky track.

8. "This again is brilliantly illuminated in the same manner, and as suddenly left a dull blank.

9. "In certain nights, they assume the appearance of vast columns; on one side of the deepest yellow, on the other, declining away till it becomes undistinguished from the sky.

10. "They have generally a tremulous motion from end to end, which continues till the whole vanishes.

11. "In a word, we, who only see the extremities of these northern phenomena, have but a faint idea of their splendour and their motions.

12. "According to the state of the atmosphere, they differ in colour.

13. "They often put on the colour of blood, and then make a most dreadful appearance *."

* *Encyclopædia Britannica.* Art. Aurora Borealis.

1. Now, with respect to the first and second of the sentences here transcribed, there seems reason to doubt the accuracy of the account which almost limits the appearances of the Aurora to the "winter time," to "frosty weather," and especially to the winter "solstice." The frequency with which the season approaching to Christmas, or that of the winter solstice, is distinguished by the occurrence of weather peculiarly mild, insomuch that, almost every year, the period is marked by observations upon what is annually called the extraordinary and unseasonable genialness of the weather, cowslips blooming, leaves budding, and birds building their nests; this frequency of a mild temperature of the air about the period of the winter solstice, may justify, even under a general view, a doubt of the accuracy with which, as things of course, the winter solstice, and frosty weather, are spoken of as arriving in conjunction. But, that the appearance of the Aurora Borealis is not peculiar, either to the occurrence of frosty weather, or to the period of the winter solstice, whether the two latter phenomena are related or otherwise, seems probable, as well from the mildness of the weather at the late appearance, as from the various seasons of the year in which the few others described in our books are recorded to have presented themselves. The earliest mentioned was seen in London in the year 1560, on the 30th day of January. The next was in 1564, on the 7th of October. The next, in 1574, on the 14th and 15th of November. The two next, observed in Brabant, in 1575, on the 25th of February, and 28th of Septemher. The next, at Wurtemburg, as we are assured by Mœstlin, seven times, in the year 1580. The next, in an extraordinary manner, in the months of April and September, 1581; and in a less degree, at some other places, in the same year. The next, observed all over France, in 1621, on the 2nd of September. The next in 1707 and 1708, during which two years the Aurora was witnessed five times. The next, in the month of March, in 1715-16. The next, in 1737, on the 16th of December; that seen in London in 1791, of the month of which the writer is uninformed; another in 1803, or 1804, at the latter of September, or the beginning of October; and this, of 1827, on the 25th of September. But, from these statements, it is now seen, that,

exclusive of appearances of the Aurora in respect of which the month is not particularised, eight of the different months of the year occur by name; that is to say, the months of September, October, November, and December, January, February, March, and April; leaving only four months (May, June, July, and August, the identical summer-months of the Polar regions, or months during which the sun visits the Polar horizon!) hitherto undistinguished by the phenomenon of the Aurora, and almost establishing, as the season of its occurrence, not the middle point of the winter solstice, but the whole period extending, in general terms, from the autumnal equinox to the vernal, beginning at or before the first, and ending at or after the last; or, what may be called the entire winter of the northern hemisphere, or the period during which the sun's course is to the southward of the tropic of Cancer; a deduction from the scanty data offered by such archives of the phenomenon as we possess, not, perhaps, of trifling importance toward the establishment of the true theory of the cause, as well as of the purpose of its being.

2. The third sentence, where it describes the Aurora Borealis as the *constant* attendant of clear evenings in the Shetland Islands, and thereby a great relief to the gloom of the long winter-nights, is probably tainted with errors in regard to the phenomenon, such as affect its whole history and philosophy. The suggestion has just been hazarded above, that at least considerable displays of the Aurora are probably almost as rare, even in the Arctic regions, as in climates further south; and the truth of this persuasion, as the writer anticipates, will fully appear below. In the sentence now referred to, the word "constant" should, at least, give way to "frequent," if not to "often;" and a distinction should be allowed for, between those feeble appearances which alone, it may be suspected, are even *often* beheld in the Shetland Islands, and those extraordinary displays which make themselves visible to their southward.

3. The fourth of the above sentences, in which the Aurora is said to appear commonly at twilight, will have been seen to agree with the time assigned for the commencement of the Aurora in the late example; and this, when coupled with the

observation in the third, that, in the Shetland Islands, it is the constant attendant of *clear* evenings, will seem to suggest, what, indeed, will probably be easily agreed to, that the Aurora, in itself, is peculiar neither to clear evenings nor to evenings at all; but is in activity during the twenty-four hours, or without intermission; though, to be visible to human eyes, first, the atmosphere must be dark, and, secondly, it must be more or less clear. It may also be thought apparent, from the terms of the twelfth and thirteenth sentences, that too much has not been said by the present writer, of the degree in which the peculiar spectacle, upon each separate occasion, depends, not alone of the proper and really uniform features of the Aurora itself, but also of the atmosphere through which it is seen, with the appearance of which its own appearance is combined; and of the consequent value of a careful separation of the real phenomena of the Aurora, from the adventitious phenomena of the intervening and surrounding atmosphere. That the colours which, whether visibly connected with the atmosphere or otherwise, are displayed during the appearance of the Aurora Borealis, are wholly derived from the atmospherical medium through which we behold it, and that the Aurora itself exhibits only a pure white light, is what the writer greatly inclines to suspect, and what may seem to be rendered still more credible by that which is reported by those who have obtained a partial glimpse of the Aurora *Australis*, or corresponding phenomenon of the south. This is described, by Mr. Forster, who sailed round the world with Captain Cook, as consisting in " long columns of clear white light;" but the whiteness, in the eyes of the narrator, seemed to establish a difference, instead of a similitude, between the Auroræ Australis and Borealis, Mr. F. wholly overlooking the explanation which his own account supplies! " These columns," says he, " though in most respects similar to the Northern Lights (Aurora Borealis) of our hemisphere, yet differed from them in being always of a whitish colour, whereas ours assume various tints, especially those of a fiery or purple hue. The sky was generally clear when they appeared, and the air sharp and cold, the thermometer standing at the freezing point." Now this text is its own commentary. The

Aurora could not have been seen if the sky had not been more or less *clear*. But the sky was *very clear*; and this because the weather was *severely frosty*. The thermometer "was standing at the freezing point." The weather was settled frosty, and therefore settled clear; for the Aurora appeared for "several following nights." The atmosphere, therefore, was clear; there was neither cloud nor fog, and thence the whiteness of the Aurora. But these views of the Aurora *Australis* were partial occurrences, and were characterised, as we must conclude, by the state of the atmosphere at a particular conjuncture, or at a particular season of the year. In point of fact, the Aurora was seen on the 16th of February, 1773, in latitude 58° S. This was the beginning of the Australian winter, and it might be a very cold, and therefore a very clear beginning. But the atmosphere of the southern half of the globe is not always thus translucent; and when it is otherwise, we may depend upon it that the columns of its Aurora "assume various colours, especially those of a fiery and purple hue," more or less like our own. A friend of the present writer was in the same latitude (58° 12′ S.) in the month of March, a few years since; and, upon asking that gentleman whether he had ever beheld an Aurora in the Southern Hemisphere, his answer was in the negative. The season of his visit, however, was a month later in the southern winter than the visit of Messrs. Cook and Forster; the weather was thick and sleety; it was unfavourable to any view of an Aurora at all; but, had the phenomenon happened to present itself, its appearance, we may believe, would not have presented that of a uniform, clear, white light.

4. In the fourth and sixth sentences, what is said of "change of shape," and "change of form," is of a nature exceedingly to mislead such as, never having themselves witnessed the phenomenon, may desire either to figure it to their imagination, or to reason upon its appearances. In reality, there is no such change of shape or form as the words naturally suggest to our ideas; the forms, under all changes, are still linear; and the actual changes, as to form, are limited to such changes only as can be produced with the single material of lines, lengthened, shortened, varied in their direction, and now fixed, now shaken, now darting; and now joined in rapid and intermingling motion.

Add, that these lines are luminous, and varied in colour from white to yellow, red, and crimson, and, sometimes, perhaps, to purple and to violet; that they play, in the lower heavens in a field of light, and in the upper over a sky of blue; and the picture of the Aurora Borealis is well nigh complete. The observation in the ninth sentence, that the vast columns, of which, upon some occasions, the Aurora displays the forms, are of a deep yellow upon one side, which, upon the other, fades gradually into that of the sky, is to be understood, as expressing, that, as in the late example, the outer edges of the columns, or those next the dark or millumined portion of the horizon, are sharp and strongly defined; while the inner ones are less distinguished from the general field of light in which they stand; and which distinction, after all, is but a delusion of the eye, which more readily distinguishes the variation of colour in the outer edge, which is so strongly relieved by the dark and colder-coloured part of the sky, than the colour of the inner part and edge of the column, which, more or less, approaches that of the ground behind it.

5. Sentences seven and eight appear to the present writer to convey the most accurate description possible, of the appearance of the Aurora in the *zenith*. The "dusky track," which remains after the lights which have enlivened it are extinguished, and in which they are so often seen again, may seem to attest the justice of his opinion, that these appearances in the zenith are no other than the far-projected tops of the columns which have their bases in, or rather below, the horizon; tops which, while they fill the southern half of the zenith, to the view of spectators under our parallel, must gradually descend toward the horizon, in the eyes of such as behold them further and still further to the south; till, like the topmast of a receding ship, they first scarcely remain discoverable above the convexity of the surface intervening, and finally dip and sink beneath it. But, upon this assumption, the appearance, and therefore office, of the Aurora Borealis, must be conceived as extending far to the southward of even our own island; and the statement, as in the eleventh sentence, becomes more or less inaccurate, that "only the extremities of these northern phenomena" are witnessed by ourselves. In reality we are

ourselves inhabitants of the Northern hemisphere; and the relationship of the Aurora to the wants of the whole hemisphere is more extended, perhaps, than we have commonly imagined. It is even a contradiction to say, as in the eleventh sentence, that we see only the extremities, that is, the Southern extremities of these Northern phenomena, after having said, in the fifth sentence, that "they often cover the whole heavens, and then make the most brilliant appearance;" unless, indeed, in both of these remarks reference is made to the spectacle beheld under more Northerly parallels, a reference which is further suggested, together with their apparent origin, in the terms of a description by Gmelin, to be cited below, of the Aurora as beheld upon the coasts of the Icy Sea. If the Aurora, there, or upon the banks of the Lena or Yenesei, is seen to rise in the north, but yet to stretch itself over the whole hemisphere, it must follow, that its "extremities," that is, its southern extremities, so far from being all that is seen in these situations, are really projected, on those occasions, so far to the southward, as to escape the ken of our northern optics; a fact of which the explanation must be familiar, inasmuch as, owing to the convexity of the surface of the globe, the horizon of every part is narrowly bounded, whether upon the South or upon the North; whence it results, that any celestial, or even* atmospherical appearance, stretching only a little way beyond us to the Southward, or toward the East, or toward the West, must soon reach the horizon upon either of those sides, and thus cover all that, to the eye of any individual, is visible of the "whole hemisphere."

6. But the description, by Gmelin, of the Aurora, as seen upon the shores of the Icy Sea, and more than all, the simplicity with which the naturalist is disposed to fix its birth-place in that precise interval of the earth's surface which divides the mouth of the river Yenesei from that of the river Lena, in the North-east of Asia, (a spot so far to the *North-eastward*, too, of our own!) while it may possibly explain the origin or bearing of remarks, that it "sometimes covers the whole hemisphere, and then makes the most brilliant appearance," will also afford something of an answer to such as, with the writer quoted above, seeking to connect the Aurora Borealis with the

Magnetic Pole, would discover its same birth-place, or focus, in the *North-west*, or nearer to the North-west of America, than to the North-east of Asia! It may furnish a reply, also, to Gmelin himself, who, though he tells us that, even upon the banks of the Lena and Yenesei, the Aurora is still seen to rise to the North or North-east of those situations, yet imagines those very banks to be its "real birth-place;" for is it not plain, in the meantime, and this from the very statement of the author, that, travel as far northward, or north-eastward, as we will, the birth-place of the Aurora still retires from our feet; that, even upon the shores of the Icy Sea, the joyous phantom is still to our Northward, or North-east, and that we may reasonably conclude, that even a voyage upon that sea would not carry us to the cradle in pursuit; that, in short, at the North Pole, we should still behold it rise in the North, or the North-east, or the North-west; that we might sail down the Western Hemisphere, and yet only discover, that the Aurora was now in the North behind our backs, as it had been before in the North before our faces; and that, in short, so long as we do but admit its existence in the North, the particular soil or sea is best described in the most general terms:—

"In Nova Zembla, or the Lord knows where!"

The search, too, for the paternal hearth of the Aurora Borealis in any particular division of the Northern Hemisphere, and especially the attempt to find it at the Magnetic or Electric Pole, is, perhaps, so much the more hopeless, after ascertaining, as above, that each hemisphere has its Aurora; and after concluding, as we may have been led to conclude with reason, that each Aurora, other things equal, resembles the other! What is remarkable, also, is that, in the Southern Hemisphere, as well as, according to Gmelin, in the Northern, it is to the Eastward, or to the East of North, that the Aurora has its apparent focus. "A beautiful phenomenon," says Mr. Forster, (Feb. 17, 1773, lat. 58° S.) "had been observed during the preceding night, which appeared again this and several following nights. It consisted of long columns of white light, shooting up from the horizon to the eastward, almost to the zenith, and gradually spreading over the whole southern part of the sky. These columns are gradually bent sideways

at their upper extremities; and, though in most respects similar to the Northern Lights (Aurora Borealis) of our hemisphere, yet differed from them in being always of a whitish colour; whereas ours assume various colours, especially those of a fiery or purple hue. The sky was generally clear when they appeared, and the air sharp and cold; the thermometer standing at the freezing point." This occasional bending of the columns, "sideways at their upper extremities," instead of uniform convergence toward the zenith, observed by Mr. Forster in the Aurora of the South, is plainly the same peculiarity which was recently witnessed in London, in the Aurora of the North, and a circumstance which, in whatever way explained, assists in the identification of the natures of the two phenomena; and, if we are still to hesitate, upon account of the whiter light of that of the South, let us believe that particular to originate in some peculiar constitution of the Southern atmosphere, from which, in one way or another, not here to be discussed, the cause of the difference may offer itself. But Gmelin's account of the Aurora of the North, to which the attention of the reader has already been called, is that which is here required to follow. It is to serve to illustrate, as will be remembered, much of the foregoing: "This Northern Light," says that author, "begins with the rising of single light pillars in the North, and almost at the same time in the North-east, which, gradually increasing, fill a large space in the heavens, rush about, from place to place, with incredible velocity, and finally almost cover the whole sky, up to the zenith: the streams are then seen meeting together in the zenith, where they produce an appearance as if a vast tent was expanded in the heavens, glittering with gold, rubies, and sapphires. A more beautiful spectacle cannot be described; but whoever should witness such a Northern Light for the first time, could not behold it without terror; for, however beautiful the illumination may be, it is attended, as I have learned from the relation of many persons, with a hissing, crackling, and rushing noise, throughout the air, as if the largest fireworks were playing off. To describe what they then hear, they make use of the expression, 'Spolochi chodjat;' that is, 'The furious army is passing!' The hunters, who, upon the confines of the

Icy Sea, follow the chase of the blue and white foxes, are often overtaken in their excursions by the Northern Light; and, upon this occurrence, their dogs are so much frightened, that they will not move, but cower obstinately upon the ground till the noise is over. The weather, after the appearance of the Northern Light, is usually clear and calm. I have heard these accounts, not from one person only; but from many of those who have spent several years in these very Northerly regions, and inhabited different countries from the Yenesei to the Lena, so that no doubt of its truth can remain; for here seems to be the real birth-place of the Aurora Borealis."

8. Upon this statement itself, it is only needful to remark, that the rising of the pillars in the North-east, or to the East of North, rather than to the North-west, or West of North, almost at the same time with their first appearance in the North, is not, perhaps, even as seen between the Lena and Yenesei, so uniformly the case as M. Gmelin may have been led to believe; and that, at all events, as above described, the progress of the late display, observed in London, was, first from North to West, and afterward from West to East; the North being always the centre, or always light, while the West and East were changed. The covering of the whole sky, and the splendour of the scene produced, have been the subject of previous remark; and the observation, "that the streams (previously called pillars) are then seen meeting together in the zenith," entirely corroborates what the present writer has said of the nature of the lights seen skimming across the zenith, and across each other, and the deduction which he has thence made, that the luminous appearances in the zenith are the summits of those very pillars of which the bases are on or below the horizon. The clear and calm weather which, on the shores of the Icy Sea, commonly follows the appearance of the Aurora is, in some degree, in concord with the phenomena of its recent appearance in London; where, without any material change in the temperature, a succession of clear, calm, and bright days supervened, within a day or two of the Aurora. As to the hissing, crackling, or rushing noise, which is said to accompany the Aurora in the more northern regions, and which has sometimes been compared to that of the furling and

unfurling of flags, there is nothing difficult, (knowing what we do of the noise of winds and of thunder,) in admitting its probability, unless what may arise from the consideration, that the noise might, or might not, be expected to be heard, wherever the phenomenon is to be seen. But the most striking and important truth, communicated in the foregoing account, is that which we cannot but rigorously infer from the collective testimony of two very distinct descriptions, which is afforded in two of the concluding sentences. It consists in that real *infrequency*, as well in the Northern, as in the Southern Hemisphere, of the appearance of the Aurora; an *infrequency* the knowledge of which is so essential to the true history of the phenomenon, and therefore to its true philosophy, and consequently to much of the history and philosophy of nature at large;—an *infrequency* which the present writer has given notice of above, as a proposition for which, in dissent from all received authorities, he will contend; and upon the opposite account of which matter, in the general account quoted, he has already requested the reader to suspend his judgment. It is obvious that, as a natural phenomenon, an Aurora Borealis, which, though *constantly* experienced in the more Northerly regions, is but rarely observed in the more Southern; that is, an Aurora Borealis which, though familiar to the Samoiede, the Laplander, and even the Shetlander, is an extraordinary, and a terrific, or at least a marvellous event, to the Italian, the Frenchman, and even to the Englishman; it is obvious, that such an Aurora Borealis, *constant* in its occurrence a little further to the Northward, and almost the solitary spectacle of a generation a little further to the South, is, as a natural phenomenon, a very different thing from an Aurora Borealis which, though far enough to the South, sufficiently frequent in comparatively trivial magnitudes and lustre, is seen, either in the South or in the North, in its greatness, and in its splendour, but yet rarely, and with, perhaps, almost equal rareness; it is obvious that, as natural phenomena, and not less so as sights connected by mankind with their own fortunes, the two things now described are exceedingly unlike as matter of history, and equally so as matter of philosophy. If we are simply to record the occurrence, it is one thing to speak of a phenomenon

which, in the South, is seen only at long intervals, while it is a "constant attendant" in the North; and another thing to speak of that which, whether in the South or in the North, is equally rare, and equally out of the "constant" course of nature. If we are to write the history of nature, it is one thing to relate, that such phenomena, or rather others, infinitely more splendid, more terrific, or more marvellous, than that which was witnessed in London, in the month of September in this year, or in the same month some three-and-twenty years ago, or else some six-and-thirty, and, to judge by experience, is not to be looked for, in the same city, during twenty or thirty years again;—it is one thing to relate that, in the Shetland Islands, such a spectacle is a "constant attendant of clear evenings," and another thing to relate, that though, perhaps, on clear evenings, in the Shetland Islands, some small displays of the Aurora are not unfrequently perceived, yet, that such an exhibition as has recently been witnessed in London, and still more, such as, more effulgent, and more extended, and more vigorous, and even coloured by the atmosphere into the terrific;—that those exhibitions, in short, of which our naturalists and men of science would persuade us, that, while beheld nightly by those of the North, they are known to us by very faint examples alone;—those exhibitions,—that those extraordinary examples of the brightness and vigour of the Aurora—are as rare, or almost as rare, not only in the Shetland Islands, but in Iceland, and on the shores of the Icy Sea, as in the streets of London themselves! It is obvious, too, that if we are to speak of this phenomenon philosophically, if we are to attempt to explain its origin and use,—its source in the natural elements, and its office in the natural economy; here, too, the solving of this question of the *frequency* or *infrequency*, the *constancy* or the *inconstancy*, of these mighty exhibitions, even in the North, and under the Pole itself, is matter of foremost importance. And what is the testimony, upon these heads, which is borne by the accounts collected by Gmelin? Is the Northern Light of the German naturalist, the apparently constant attendant of clear evenings, even in the countries between the Lena and the Yenesei? Is the spectacle, and the atmospherical hurley, which seems to rush over the

hunting-grounds of the hunters and their dogs, and which frightens the very dogs, and pins them to the ground till it is passed, or has seemed to pass; is this the "constant attendant of clear evenings," or, is it a prodigy so uncommon as to defy familiarity? But, if this evidence is insufficient, let us look to what is said of its influence, in these countries, on the subsequent state of the atmosphere. So far from the Aurora being an attendant or follower of clear evenings, it seems that clear evenings follow the Aurora! It is said, that after its occurrence, clear and calm weather is customary to follow; and, here, the expression itself is implicative of the rarity of the occurrence. If it were constant, how should this result come to be noticed; and, indeed, if the Aurora Borealis were the constant attendant of each twenty-four hours, and if clear weather were usually in the train of the Aurora Borealis, how could it ordinarily happen, that there should be any thing else than clear weather, in the countries visited by the Aurora, or any foul weather for the Aurora to dispel? Yet, such is the established prejudice concerning this supposed frequency of the more powerful displays of the Aurora in the climates further to the North than our own, that a writer, quoting the very statement above, absolutely prefaces it with the remark, that Gmelin, in pointed terms, speaks of the Aurora as "frequent," as well as "very loud," "in the North-eastern parts of Siberia*!" A simple perusal, in the meantime, is sufficient to show, that Gmelin says nothing affirmative as to its *frequency;* while a slight consideration of the facts which he adduces must satisfy us, as no doubt they satisfied Gmelin himself, that the occurrence, even in Siberia, is actually *infrequent!*

XI. In reference, however, as well to the image presented above, of "a vast tent expanded in the heavens, glittering with gold, rubies, and sapphires;" as also to many less ambitious and figurative descriptions of the spectacle of the Aurora Borealis, (not excepting that indicted by himself,) the author is anxious to suggest a caution against the too exaggerated conception of the realities intended. Words,

* Encyclopædia Britannica.

upon such occasions, are rarely more than imperfect pictures, presenting but feeble likenesses, and either deficient or excessive in the amount of beauty, or of the reverse, of whatever kind, which they attempt to copy from their originals; and the inconvenience is seriously great, whenever the object portrayed is wholly strange to the mind before which it is placed. The imperfect power, both of words and written characters, to convey precise, and sometimes even tolerable ideas, of the objects, either sensible or abstract, which they are intended to represent, and the superior intelligibility so often belonging to diagrams or figures, or other resources of the art of drawing, (the primitive, and, for so many purposes, the most instructive mode of writing *,) would have led the present writer, had time permitted, to endeavour, as often as possible, to elucidate by such means the several parts of the foregoing observations; but which means, at last, and in reference to the actual phenomena of the Aurora, would necessarily fail to convey the due, and yet no more than the due impression, to such as are wholly without its ocular acquaintance. We are little aware how much, upon ordinary occasions, our understanding of words heard or read is assisted by our previous knowledge of the sensible objects, or of the acquired notions, to which they refer; and the examples would be endless, of the sensible objects preposterously misconceived, as well as the propositions made false or ridiculous, through the frequent inadequacy of words to communicate truths entirely new

* The individual, social, and political importance of making the *art of drawing* a branch of general education, is a subject which the author can never cease to urge upon the attention of his fellow-countrymen, and of all the civilised world. It is more than ten years since he first endeavoured to lead the public eye to its regard. In England, and with a view to the subsistence of a large and always increasing population, it is an EDUCATION IN THE ARTS which is the great want; and the *art of drawing*, besides being the assistant of all *knowledge* whatever, is peculiarly so of all other *arts* than itself, or of all other works of the *hand*. A recent Sermon, by the Lord Bishop of Bath and Wells, preached at Wells, for the benefit of the Diocesan National Schools, bears ample testimony to the deficiency, and even the dangers, to the poor not less than to others, in all the present popular education; and, so far, therefore, to the soundness of the author's principles, and to the fitness of his remedy. His own design, however, is not only to remedy an evil arising from the present practice, but also to produce an independent good; and, not merely to aid the poor, nor merely to promote the political welfare of this kingdom, but to increase the resources, physical and intellectual, of all classes, and to promote the welfare of the whole world.

to the disciple. Even the history of opinions concerning the Aurora Borealis itself, might be cited upon this very point.

The ordinary and natural resource, in such circumstances, is comparison; but even comparison has been the source of great and endless errors of description. Of the degree of resemblance proposed between the known and the unknown, there is no common measure for the minds of the hearer and the listener, and the point or points of comparison intended by the first must often be mistaken by the second; or, if reference is made to a similitude under one aspect, the imagination conceives a resemblance also under another: thus, if it is said, that an unknown animal is as *large* as a horse, the idea of the *figure* also of a horse, is apt to be attached. A modern English work of science premises, upon the subject of the Aurora Borealis, that its appearance is so well known as to render description needless. It is true that the work referred to is printed in the Northern part of the island, where the phenomenon is doubtless more familiar than in the Southern; but, in the foregoing pages themselves, it has, perhaps, been demonstrated as probably certain, that if it is any where sufficiently known to render description trite for the common eye, it has at least never hitherto been described with sufficient precision for the aid of speculative research. To attempt to explain its *cause*, and to relate its entire history, its *appearance* must first be either observed or described with accuracy; and we have seen, above, that some of the most scientific reasonings which have hitherto been offered as to the former, are wholly inapplicable to the true peculiarities of the latter.

Considered simply as a visual object, and as a meteor differing from all others, and especially from all other luminous meteors, in this, that its duration extends to hours, if not to days and months; the only resemblance, perhaps, that can be suggested, is to that description of lightning which is called *heat-lightning*, the frequent companion of our summer-evenings. But, here, the similitude is inexpressibly feeble; since heat-lightning has nothing, either of the splendour, the volume, or the beauty of the Aurora; and since the light of the latter, however mobile, varied, and, from time to time, increased and diminished in itself, is yet, as to general effect, continuous and

steady. There remains, then, but to compare the phenomenon of the Aurora with the rising or the setting Sun. In both of these latter, as in the Aurora, the light is in the horizon, and that light is shot upward, perpendicularly, or obliquely, toward the zenith or toward the right and left; and both of these, like the Aurora, are more or less constantly attended with a variety of colouring, similar in hue if not in depth, and always beautiful, and often gorgeous. With the *Sun*, and with the beams of the Sun, ancient description, in point of fact, has confounded the *Aurora Borealis*, to the degree, perhaps, of giving origin to some of the ancient and poetical descriptions of the Sun, utterly inappropriate and inexplicable as understood of that day-star, but easily recognised in the Aurora; yet the dissimilitudes, at last, are numerous and great! Of the essential difference of figure, both as to the *beams* of the Sun, and the *beams* of the Aurora, in severalty, and of the inevitable difference of indication of which, as to their nature, mention has been already made; and also as to the general or collective figure of the beams of the Aurora, as contrasted with that of the rising, or of the setting Sun. The next point is the homogeneity of colour in the beams of the Sun, however the apparent colour may vary, as it is seen to do, from horizontal stratum to horizontal stratum, from the horizon to the zenith, according to the varied density of the medium between the light and the eye of the spectator. The light, upon the other hand, of the beams of the Aurora is heterogeneously coloured in itself, and is so displayed; and not, therefore, varied as the beams ascend from horizontal stratum to horizontal stratum, or as crossing all the beams together, but found in each particular beam itself, and attending its direction, whether vertical or inclined, and whether rectilinear or curved. Waiving, then, any comparison in detail, between the phenomena of the Aurora, and the phenomena of the rising or of the setting Sun, but admitting that, to a certain degree, all are alike vast in dimensions, splendid in light, rich in colour, and durable upon the eye; there is still nothing else to be subjoined, than that, at least with reference to vastness of dimension and magnitude of the volume of light; to the quantity of light diffused; and to the richness and gorgeousness of

the attendant colouring; there can be little risk in the assertion that, vast, and splendid, and beautiful, and rich, and gorgeous, as, when seen in the most favourable situation, and under the most favourable circumstances, the Aurora may be, it is, at last, but insignificant, when compared, for those features, to the vastness, the splendour, the beauty, the richness, and the gorgeousness, more or less, from day to day, displayed in the rising or the setting of the Sun; and, that for chaster beauty, and even for amount of light diffused, it is not even to be likened to the silver Moon! As a substitute, too, for either, or for both, the Aurora, in the regions of cold and night, may justly demand the admiration and the blessing of mankind; and, in regions cold and inclement, its rarity, not unaccompanied by beauty, by grandeur, and sometimes even by the terrible in appearance, may well invite the gaze and fix the attention of beholders; but, considered along with the light of the luminaries of heaven, its claims reduce themselves in quality, though certainly not in degree, to a level with those of an artificial lustre; and we almost repeat, in reference to the light of the Aurora, as compared with that of the Sun, or even of the Moon, what the poet has said in reference to the lights of our chambers:—

> "Who but rather turns
> To heaven's broad beam his unconstrained eye,
> Than to the glimmering of a waxen flame?"

The Moon, in the meantime, inferior as she is to the Sun, has been "blessed," from age to age, for her "useful light;" and the "useful light" of the Aurora, also, has its claims to "blessing." It co-operates with the Sun, the Moon, and with other agents of nature, to make, not merely the Polar regions of the earth, but the entire globe of the earth, fruitful, at once, and habitable*!

* The author has an opinion, that among the "agents of nature," for equalising the temperature of the surface of the globe, is to be reckoned, not only the Northern and Southern Lights, but the entire Ocean; and that this agency is the immediate object aimed at in the existence of this last, as one body of water surrounding the entire globe. His evidence consists in geographical, hydrographical, meteorological, and physiological facts, as also in the apparent reason of the case. He supposes, in consequence, a perpetual circulation of the waters of the sea, longitudinally round the globe, or from North to South, and from South to North again; and the result of Captain Parry's late attempt to reach the Arctic Pole, as also some of the facts which have transpired respecting Captain Franklin's late land expedition, appear to confirm

XII. In a succeeding paper, the author may possibly submit to the consideration of his reader, the particular and novel hypotheses which he has allowed himself to form, as to the *substance*, *causes*, and *effects* of the Aurora; hypotheses partly dependent upon those *facts in its natural history*, which, above, have been almost the exclusive objects of attention. At present, the leading particulars of the *natural history* of the phenomenon, which it has been attempted either to bring or to fix in view, are these:

1. That the Aurora is a phenomenon observed both in the Northern and Southern Hemispheres.

2. That, in either hemisphere, it is observed in the general direction of the corresponding *pole of the earth*.

3. That, in the Northern Hemisphere, on the shores of the Icy Sea, or at the furthest distance north, its situation is still observed to be the northward.

4. That, in the Southern Hemisphere, it has been observed to the east of the South Pole, and in the Northern, to the east and west of the North Pole.

5. That, upon the late occasion, the place of its columns, during the exhibition, was observed to change from the west of north to the east of north; but, so as always to have the north for the apparent centre of its strength.

6. That, in the Arctic regions, the appearance of the Aurora is said to be usually followed by clear and calm weather.

7. That the appearance of the Aurora Borealis is no wise peculiar to the winter solstice, but has been observed in each of the eight months of September, October, November, December, January, February, March, and April, and may be regarded, therefore, as coincident with the Arctic winter; and that the appearance of an Aurora in the Southern Hemisphere, in the month of February, or beginning of the Antarctic winter, as observed during the voyage of Captain Cook, in the year 1773, is

his theory, according to which the physical use, or final cause, of the existence of the Ocean has never previously been understood. His theory affects the question of the North-west Passage, which latter object he suspects to have never yet been pursued in the true direction; even the discoveries of Captain Parry appearing to him to have fallen short of ascertaining the communication with the Polar Sea by the channel of Davis's Strait.—Some introductory observations upon this subject have been already made in an article in the New Monthly Magazine for October, 1826, (vol. xvii. p. 371.)

consistent with the persuasion, that the Aurora *Australis*, in its turn, is a phenomenon of the Austral or Antarctic winter.

8. That *considerable* or powerful displays of the Aurora are infrequent, even in the extreme Polar regions; and that it is *very considerable* or powerful displays alone, which make themselves visible in the lower latitudes, north or south of the equator.

9. That no appearance belongs to the Aurora itself, but that of its coruscations, columns, spears, or streamers; and that all colours, therefore, or coloured figures, not belonging to the coruscations, but coincident in their appearance, are to be regarded only as reflections or refractions of light, derived from the coruscations by the clouds which happen to cover the sky.

10. That the colours, or coloured light, proper to the Aurora, or seen in the columns or coruscations themselves, are varied from column or coruscation to column or coruscation, and severally continued in the direction, and throughout the length or height, of each.

11. That, in the late example, the columns or coruscations situate in the due north, or apparent centre or focus of the phenomenon, exhibited a light at least comparatively white; and that the variation, from white to colour, had an apparent relation to the comparative remoteness of each column or coruscation from the column or coruscation in the central north.

12. That the direction or position of the columns or coruscations of the Aurora, are so far from being uniformly convergent toward the zenith, or uniformly vertical, or from the horizon to the zenith, that, in the late example, they *did not converge* toward the zenith, but, contrariwise, *diverged* from it; spreading themselves like the sticks of a fan, or like stalks in a flower-basket.

13. That the columns or coruscations of the Aurora are not uniformly rectilinear in their figure; but that, in the late example, those on the north-eastward were curved outwardly, or "bent sideways," as described in the appearance of the columns or coruscations of an Aurora seen in the Southern Atlantic, during the voyage of Captain Cook, in the year 1773.

Proceedings of the Royal Society.

The anniversary meeting of the Royal Society for the election of a president, and other officers, was held as usual at Somerset House, on Thursday, the 30th of November, being St. Andrew's day.

Till within a few days of the election, it was generally understood that the Rt. Hon. Robert Peel was a candidate for the chair; in consequence, however, of that gentleman having declined, Davies Gilbert, Esq., M.P., was put in nomination, and was almost unanimously elected the President of the Royal Society.

The late secretaries, Messrs. Herschel and Children, having resigned their respective offices, Dr. Roget and Captain Sabine were nominated in their places, and were duly elected.

The accession of Mr. Gilbert to the chair having rendered vacant the office of Treasurer, Major Kater was elected in his place.

The following council was elected, to continue in office until St. Andrew's day, 1828.

Davies Gilbert, Esq.
Major Kater.
Dr. Roget, M.D.
Captain Sabine, R.A.
Dr. Wollaston, M.D.
Dr. Fitton, M.D.
Dr. Young, M.D.
Dr. Paris, M.D.
Dr. Prout, M.D.
Dr. Goodenough, D.D.
Dr. Buckland, D.D.
J. W. Croker, Esq.
Lord Colchester.
Sir E. Home, Bart.
Sir H. Davy, Bart.
John Pond, Esq.
Capt. F. Beaufort, R.N.
Francis Baily, Esq.
John Guillemard, Esq.

In consequence of having been elected President *pro tempore* by the council,) the chair having been vacated some weeks previous to the general election,) the duties of the office were performed by Mr. Gilbert, on occasion of the present anniversary. After having read over the list of members admitted, and of those deceased during the last year, he proceeded to announce the disposal of the Royal and Copley Medals, as awarded by the votes of the council.

Of the Royal Medals, one was awarded to Sir H. Davy, and the other to Professor Struve. Of the Copley Medals, one was given to Dr. Prout, and another to Lieutenant Forster. On this occasion Mr. Gilbert pronounced an eulogium upon the respective receivers of the medals; and, in adverting to the labours of the several individuals, he justified the decision of the council, in bestowing upon them these marks of distinction, in a learned and eloquent discourse.

Proceedings of the Horticultural Society.

September 4th.

A paper by Mr. Lindley was read upon the new hardy plants which had flowered in the Society's garden; among them a number of new shrubs were mentioned, which appeared likely to prove acquisitions to the public. A thermometer was exhibited by Mr. Bregazzi, of Derby, for ascertaining the temperature of bark-beds. It consisted of a thermometer enclosed in a shaft of copper with a wooden handle, and a door in its side, by which the temperature can be ascertained with precision. It is needless to point out the superiority of this plan, over the common mode of determining this very essential point, by feeling of a stick previously stuck in the bed; the sensation of heat when the stick is grasped in the hand will obviously depend in a great degree upon the temperature of the hand itself. As usual, there was an extensive display of all the choicest flowers and fruit of the season. One hundred and seventy-two subjects of this description were placed upon the table. Among the flowers, the most remarkable was a new hardy climber from Mexico, with deep purple blossoms studded

with glittering green glands, called Maurandya Barclaiana; among the fruit was a fine specimen, from Lord Grantham's garden, of the Papaw, a tropical fruit never ripened in England before.

September 18th.

The exhibitions of this day were chiefly confined to a display of Dahlias, which for magnificence exceeded any thing of the kind we ever witnessed before. The large meeting-room was filled with masses of the richest and most lively colours. In the whole, eight hundred and fifty-one varieties were shown, among which the finest were from the garden of William Wells, Esq., of Redleaf; but where all are so excellent, it is almost invidious to particularize. The time will be remembered by many of our readers when gardens in the autumn contained little besides marigolds, sun-flowers, and sweet-peas; by the aid of dahlias and chrysanthemums the autumn has now become the liveliest season of the year, and the beauty of the flower-garden is only destroyed by the severest of the winter frosts. Among the grapes upon the table was a remarkably excellent yellow-berried kind, from Portugal, from the garden of Mr. Holford, of Hampstead, which was quite new to this country. Apples, nectarines, peaches, and pine-apples abounded.

October 2nd.

Among the flowers was a fine bunch of ranunculuses, from Mr. Groom, of Walworth, a rare sight in October; they were obtained by having been planted in July and carefully protected by tulip-shades when coming into flower. The season for softer fruits being nearly over, pears and apples formed the chief display; of these a vast number, upwards of one hundred and eighty of the latter, were upon the table: the Blenheim orange, or Woodstock pippin, pomme gris, scarlet nonpareil, court-pendu plat, golden reinette, and packhorse apples; and Chapman's, Marie Louise, and brown beurré pears, appeared to us to excel all their rivals. The famous gloux morceau and beurré d'Aremberg pears were also exhibited, but were not ripe.

October 16th.

The first number of a new periodical work, called the *Pomological Magazine*, consisting of coloured figures of the fruits cultivated in Great Britain, was placed upon the table. Among the apples were specimens of a variety sent from England to Connecticut, in the year 1636, and reimported from America within a few years. It proved to be a kind not known at the present day in this country, but still cultivated in France. In the gardening books of the sixteenth and seventeenth centuries it is mentioned under the name of the haute bonté. The specimens exhibited served to disprove the opinion that many of the American apples are European kinds altered by climate; these, although the produce of trees which have been growing in America for nearly two hundred years, differed in no respect from French samples exhibited at a subsequent meeting of the Society.

November 6th.

An excellent paper was read upon the method of cultivating horse radish, in Denmark. The roots are cut into slips, and planted *horizontally*, the lower end inclining a little upwards, and the crown of the plant hanging over the alleys, by which the beds are separated. From time to time the roots are uncovered, and all the *lateral* fibres are carefully removed, by which the size and length of the roots are much increased. The place hitherto occupied by dahlias, was now taken by Chinese chrysanthemums, of which a large number, consisting of twenty-two different varieties, was exhibited at the bottom of the room.

November 20th.

Cuttings of the fine new Portugal grape, of which fruit was exhibited on the 18th of September, were distributed to the members present. A few dahlia flowers still showed themselves, notwithstanding the unusual severity of some early frosts, and the room was crowded with chrysanthemums. The gloux morceau and beurré d'Aremberg pears were tasted, and found to retain the station which has been assigned to them at the head of all known varieties.

ASTRONOMICAL AND NAUTICAL COLLECTIONS. For Jan. 1828.

i. Ephemeris *of the periodical* Comet *for its Return in* 1828, *computed with the consideration of a* resisting Medium. *By* Professor Encke.

Elements.

Mean anomaly 1829 Jan. 9.72, mean time at Paris, = 0° 0′ 2″.83
Mean daily sidereal motion = 1069″.87572.

		°	′	″	
Longitude of the perihelion	=	157	17	26.2	Mean Equinox 1829 Jan. 9.72.
Ascending node . .	=	334	28	47.1	
Inclination	=	13	20	47.9	
Angle of the eccentricity	=	57	38	25.2	

Ephemeris.

Mean Parisian time, 1829.		A. R. °	′ ″	Decl. N. °	′ ″	Log. Dist. ☉	Log. Dist. ⊖
Aug.	23.3	26	50+..	22	42+..	.34603	.19571
	24.3		50...		52	.34411	.18983
	25.3		49...	23	1	.34217	.18390
	26.3		48...		10	.34022	.17791
	27.3		46...		19	.33825	.17187
	28.3		44		29	.33626	.16577
	29.3		41		38	.33425	.15962
	30.3		37		47	.33222	.15341
	31.3		33		56	.33017	.14714
Sept.	1.3		28	24	6	.32810	.14082
	2.3		22		15	.32602	.13444
	3.3		16		24	.32392	.12801
	4.3		9		34	.32180	.12153
	5.3		1		43	.31966	.11499
	6.3	25	53		52	.31749	.10839
	7.3		44	25	2	.31531	.10174
	8.3		34		11	.31310	.09504
	9.3		23		20	.31087	.08829
	10.3		11		29	.30862	.08148
	11.3	24	58		38	.30635	.07462
	12.3		45		47	.30406	.06771
	13.3		30		56	.30174	.06075
	14.3		15	26	5	.29940	.05375
	15.3	23	58		14	.29704	.04670
	16.3		41		23	.29465	.03961

Mean Parisian time. 1829.		A. R.	Decl. N.	Log. Dist. ⊙	Log. Dist. ⊖
Sept.	17.3	23° 22′+″..	26° 32′+″..	.29224	.03247
	18.3	2	41	.28980	.02529
	19.3	22 41	49	.28733	.01806
	20.3	19	58	.28484	.01080
	21.3	21 56	27 6	.28232	.00350
	22.3	32	14	.27978	.99617
	23.3	6	22	.27721	.98881
	24.3	20 39	30	.27461	.98142
	25.3	10	37	.27198	.97400
	26.3	19 40	45	.26933	.96656
	27.3	9	52	.26665	.95910
	28.3	18 37	58	.26393	.95162
	29.3	3	28 5	.26118	.94413
	30.3	17 27	11	.25840	.93663
October	1.3	16 50	17	.25559	.92913
	2.3	16 11	22	.25275	.92164
	3.3	15 31	27	.24987	.91415
	4.3	14 49	32	.24696	.90668
	5.3	5	36	.24402	.89923
	6.3	13 20	39	.24104	.89181
	7.3	12 34	42	.23803	.88442
	8.3	11 45	44	.23498	.87707
	9.3	10 55	46	.23189	.86976
	10.3	4	47	.22876	.86251
	11.3	9 10	47	.22559	.85532
	12.3	8 15	46	.22238	.84820
	13.3	7 19	45	.21913	.84116
	14.3	6 21	43	.21584	.83421
	15.3	5 21	39	.21251	.82735
	16.3	4 20	35	.20913	.82059
	17.3	3 18	30	.20570	.81394
	18.3	2 14	24	.20223	.80741
	19.3	1 9	17	.19871	.80101
	20.3	0 3	9	.19515	.79474
	21.3	358 56	0	.19154	.78861
	22.3	357 47	27 49	.18787	.78263
	23.3	356 38	38	.18415	.77681
	24.3	355 28	25	.18038	.77116
	25.3	354 17	11	.17656	.76568
	26.3	353 5	26 56	.17268	.76037
	27.3	351 53	40	.16874	.75525
	28.3	350 40	22	.16475	.75031
	29.3	349 27	4	.16069	.74557
	30.3	348 14	25 44	.15697	.74102
	31.3	347 1	23	.15239	.73668
Nov.	1.3	345 47	1	.14814	.73253
	2.3	344 34	24 38	.14382	.72858

Mean Parisian time, 1829.		A. R.		Decl. N.			Log. Dist.	
		°	′ ″	°	′ ″		⊙	⊖
Nov.	3.3	343	21 +..	24	14 +..		.13943	.72483
	4.3	342	9	23	49		.13497	.72128
	5.3	340	56		23		.13044	.71793
	6.3	339	44	22	56		.12583	.71477
	7.3	338	33		29		.12114	.71180
	8.3	337	23		0		.11638	.70902
	9.3	336	13	21	31		.11153	.70642
	10.3	335	4		1		.10660	.70399
	11.3	333	56	20	31		.10158	.70172
	12.3	332	48		0		.09647	.69961
	13.3	331	41	19	29		.09126	.69765
	14.3	330	36	18	57		.08595	.69583
	15.3	329	31		25		.08055	.69415
	16.3	328	27	17	52		.07505	.69258
	17.3	327	23		19		.06944	.69113
	18.3	326	21	16	46		.06372	.68979
	19.3	325	19		12		.05788	.68854
	20.3	324	18	15	39		.05193	.68738
	21.3	323	18		5		.04585	.68630
	22.3	322	19	14	31		.03965	.68529
	23.3	321	20	13	56		.03331	.68434
	24.3	320	21		22		.02684	.68345
	25.3	319	23	12	47		.02022	.68261
	26.3	318	26		12		.01346	.68182
	27.3	317	28	11	37		.00655	.68106
	28.3	316	31		2		.99948	.68034
	29.3	315	34	10	26		.99225	.67964
	30.3	314	37	9	50		.99484	.67897
December	1.3	313	40	9	14	N.	.97726	.67833
	2.3	312	43	8	37		.96949	.67772
	3.3	311	45	8	0		.96153	.67713
	4.3	310	48	7	23		.95336	.67657
	5.3	309	49	6	45		.94499	.67605
	6.3	308	50	6	6		.93640	67556
	7.3	307	50	5	27		.92759	.67513
	8.3	306	49	4	47		.91854	.67476
	9.3	305	47	4	6		.90924	.67446
	10.3	304	44	3	24		.89969	.67425
	11.3	303	40	2	42		.88987	.67415
	12.3	302	34	1	58		.87978	.67418
	13.3	301	27	1	13		.86939	.67437
	14.3	300	18	0	27	N.	.85870	.67475
	15.3	299	7	0	19	S.	.84770	.67536
	16.3	297	54	1	8		.83638	.67623
	17.3	296	40	1	58		.82474	.67742
	18.3	295	23	2	49		.81276	.67896
	19.3	294	5	3	42		.80042	.68091

Mean Parisian time, 1829.		A. R.	Decl. N.	Log. Dist. ☉	Log. Dist. ⊖
		° ′ ″	° ′ ″		
Dec.	20.3	292 45+..	4 36+..	.78771	.68333
	21.3	291 23	5 32	.77465	.68627
	22.3	289 59	6 29	.76123	.68980
	23.3	288 34	7 27	.74746	.69399
	24.3	287 8	8 27	.73334	.69889
	25.3	285 42	9 27	.71890	.70456
	26.3	284 15	10 28	.70416	.71107
	27.3	282 48	11 30	.68917	.71845
	28.3	281 23	12 32	.67399	.72677
	29.3	280 0	13 34	.65871	.73604
	30.3	278 39	14 36	.64342	.74629
	31.3	277 21	15 37	.62830	.75750
1830 Jan.	1.3	276 9	16 37	.61348	.76969
	2.3	275 2	17 36	.59920	.78278
	3.3	274 1	18 33	.58572	.79670
	4.3	273 9	19 29	.57332	.81139
	5.3	272 25	20 22	.56231	.82673
	6.3	271 50	21 12	.55304	.84259

The opposition to the sun will be 1828, Oct. 12.34: while its light is weak, it may be observed on or near the meridian.

On the 10th of Nov. 1828, its distance from the sun will be the same as at the time of its discovery in 1818, and it will be considerably nearer to the earth; and on the 21st of December, its position with respect to the sun will be the same as at its last observation in 1819; and with respect to the earth, its situation will be more advantageous. The 1st of January, 1829, it will set with the sun.

It follows, that the most advantageous time for seeing it will be during the whole of November, and the first 25 days of December. It will scarcely be seen before the end of September, as it has heretofore never been observed more than two months before the time of its perihelion, and even in the dark winter nights will scarcely be visible more than 14 or 15 weeks before that period. After the perihelion it will not be visible in these parts of the world.

ii. *Elementary View of the* Undulatory *Theory of Light. By* Mr. Fresnel.

[Continued from the last Number.]

In order to complete the explanation of the conditions necessary for the formation of the fringes, it remains to show why a small luminous point must be employed in experiments on diffraction, and not an object of any considerable dimensions. If we resume the case of the interior fringes of the shadow of

a narrow body*, it will be easy to apply similar arguments to other cases of diffraction.

The middle of the central band, which is always formed by the simultaneous arrival of rays, which depart at the same instant from the luminous point, must be found in the plane drawn through this point and the line bisecting the narrow body: because, since every thing is symmetrical on each side of this plane, the rays which unite in it must have passed through equal routes on each side, and must consequently arrive at the same instant, unless they have passed through different media, which is not the case to be considered at present. The situation of the middle stripe being determined, that of every other stripe must also be determined accordingly. Now it is evident that if the luminous point should change its situation a little, and be moved to the right, for example, the plane, which has been supposed, would incline to the left, and would carry with it all the fringes which accompany the middle stripe. And if, instead of supposing such a motion, we suppose the luminous point to become of sensible dimensions; the integral points of which it is composed will each produce a group of fringes, and their situations will be so much the more remote as the luminous object is larger; and ultimately, if its size is sufficiently increased, they will extinguish each other and disappear. This is the reason that, when the rays cross each other at sensible angles, as in all the phenomena of diffraction, it becomes necessary to employ a very fine luminous point, in order to discover their mutual influence: and the point must be so much the finer as the angle formed by the rays is greater.

However minute the luminous point may be, it is always composed, in reality, of an infinite number of centres of oscillations, and it is of each of these centres that we must understand what has been said of a luminous point. But as long as they are very near to each other in comparison with the breadth of the fringes, it is obvious that the different groups of fringes which they produce, instead of mixing with each other in a confused manner, will be superposed almost exactly, and instead of extinguishing, will co-operate with each other.

When the two systems of waves which interfere are parallel, the interval which separates their corresponding points must remain the same for a great portion of the *surface of the waves*, that is to say, in other words, the fringes will become almost infinite in breadth, so that a very considerable displacement of the centre of undulation will cause very little difference in the agreement or disagreement of their vibrations. And in this case it is no longer necessary to employ so small an object in order to perceive the effects of their mutual influence.

If the coloured rings, which are produced by the interference of two systems of undulations nearly parallel, exhibit, like the fringes, and often within a very short distance, alternations of dark and bright stripes; this circumstance depends entirely on the want of uniformity in the thickness of the plate of air interposed between the glasses, which causes a variation of the difference of the routes of the rays reflected at the first and at the second surface of this plate, of which the mutual interference produces the bright and dark rings.

We shall readily be able to understand why the luminous rays, although they always exert a certain influence on each other, exhibit it to the eye so seldom, and in cases so much limited, if we consider that it is necessary, for such an exhibition, first, that the rays concerned shall have been derived from a common source; secondly, that the difference between their paths shall amount to a limited number of undulations only, even when the light is as homogeneous as possible; thirdly, that they shall not intersect each other at too great an angle, because the fringes would become so small as to be invisible even with the assistance of a strong magnifier; and fourthly, unless the rays are nearly parallel, that the luminous object should be of very small dimensions, and the smaller in proportion as the inclination of the rays is greater.

It has been thought necessary to insist so much at length on the theory of interferences, because of its numerous applications to the calculation of the most interesting of the laws of optical phenomena. These considerations may perhaps appear at first somewhat delicate and difficult of comprehension, notwithstanding the minuteness of the expla-

nation; but with some reflection it will be found that nothing can be simpler than the principles on which they are founded, and their application will soon become familiar to the imagination.

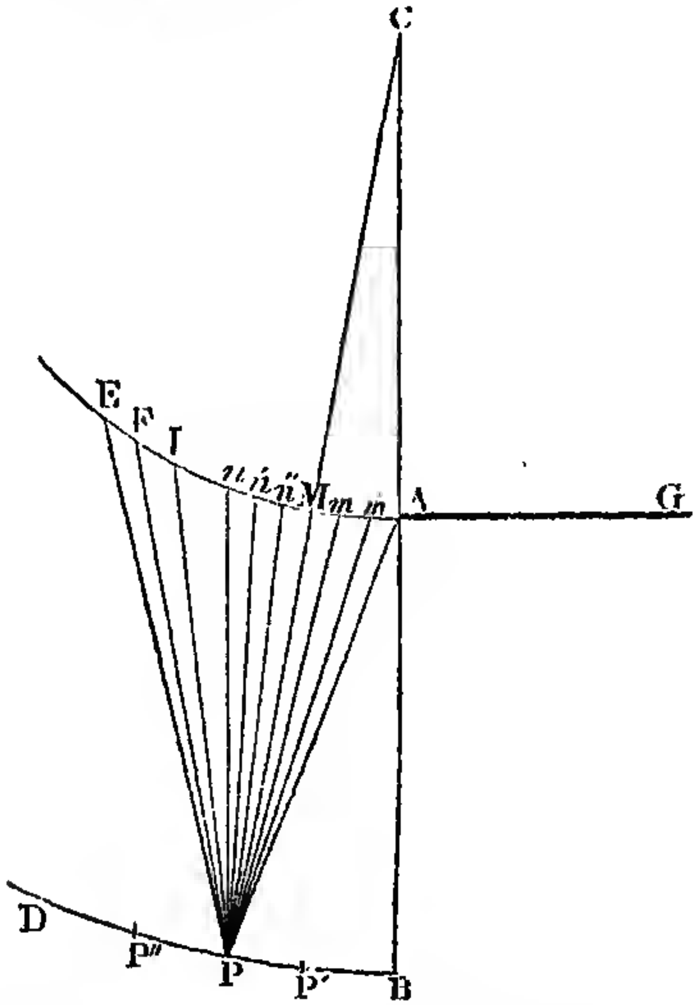
In order to complete the bases of the general theory of diffraction, it remains for us to consider the principle of Huygens, which appears to be a rigorous consequence of the system of undulations.

The principle may be thus expressed: *The vibrations of a luminous undulation, in each of its points, may be regarded as the result of the elementary motions which would be transmitted to that point, at the same instant, from all the points of the undulation, considered separately, as they existed in any one of its earlier situations.*

It is a consequence of the principle of the co-existence of small motions, that the vibrations, produced at any point of an elastic fluid, by several agitations, are represented by the result of all the velocities belonging to that point at the same instant, as derived from the different centres of the undulations, combined according to the laws of motion, whatever may be the number and situation of the centres, and whatever the periods and nature of the undulations. This general principle is applicable to every particular case. We may suppose the agitations infinite in number, of the same kind, simultaneous, and taking place in contiguous points of a plane or a spherical surface: it will also be convenient to suppose the motions of the particles to take place in the same direction, perpendicular to the surface, their velocity being proportional to the condensation of the medium, and none of them retrograde in their direction. In this manner a derivative undulation will be produced by the union of these agitations, and the principle of Huygens may be truly applied to such a propagation. [This may be called a rigorous consequence of the system, but it can scarcely be considered as a proposition mathematically demonstrated: and the fundamental law of Huygens must perhaps be assumed as an axiom or a phenomenon. Tr.]

The intensity of the primitive undulation being uniform throughout the surface, it results from this "theoretical"

consideration, as well as from other reasoning, that the uniformity will be preserved throughout the progress of the undulation, unless any part of it be intercepted or retarded; because the result of the elementary motions, which have been mentioned, will be the same for all the points. But if a portion of the undulation be intercepted by the interposition of an opaque body, then the intensity of each part will vary according to the distance from the margin of the shadow, and these variations will be particularly sensible in the neighbourhood of the tangent rays.



Let C be the luminous point, AG the screen, and AME the wave, arrived at A, and partly intercepted by the opaque body. We may suppose it to be divided into an infinite number of small arcs, Am', $m'm$, mM, Mn'', $n''n'$, $n'n$, and so forth. In order to find its intensity at the point P, belonging to any subsequent situation of the undulation, BPD, we must find the result of all the elementary agitations which each of these portions of the primitive undulation would produce there if they acted separately.

The impulse, which has been given to every part of the

primitive undulation, being perpendicular to its surface, the motions of the particles of ether in this direction must be more considerable than in any other; and the rays depending on these motions, if separately considered, would be so much the weaker as they deviated the more from this direction.

The investigation of the law by which their intensity would be governed, according to their direction, as derived from any separate centre of agitation, would certainly be of very difficult investigation: but happily we are not obliged to determine this law, for it is easy to see that when the inclination to the perpendicular is considerable, the effects of the different rays must very nearly destroy each other: so that these rays, which sensibly affect the quantity of light received at each point P, may safely be regarded as being equal in intensity.

When the centre of agitation has undergone a condensation, the expansive force tends to urge the molecules in every direction; and if they do not perform a retrograde motion, it is only because their initial velocities forwards destroy those which the expansion of the condensed fluid would otherwise generate backwards: but it does not follow from this that the agitation can only be propagated in the direction of the initial velocities; for the expansive force in a perpendicular direction, for example, will combine with the primitive impulse without any diminution of its effects. It is obvious that the intensity of the undulation thus produced may vary much at the different points of its circumference, not only from the nature of the initial impulse, but also because the condensations are not subject to the same law on every side of the centre of the agitated part [?]. But the variations of the intensity of the derivative undulation must necessarily be subjected to a law of continuity, and may consequently be considered as insensible in a very small angular interval, especially in the neighbourhood of the perpendicular to the surface of the primitive undulation; for the initial velocities of the molecules, referred to any given direction, being proportional to the cosines of the angles made by that direction with the perpendicular, these results vary much

more slowly than the angles themselves, while they remain inconsiderable.

If, in fact, we consider rays sensibly inclined to each other, such as EP, FP, IP, meeting in the point P, which we may suppose at the distance of a great number of breadths from the undulation EA: and if we take two arcs, EF and FI, of such a length that the differences EP—FP and FP—IP may be equal to half an undulation: on account of the marked obliquity of the rays, and of the smallness of a semiundulation, in proportion to their length, these two arcs will be almost equal, and the rays which come from them to the point P will be nearly parallel; so that on account of the difference of a semiundulation between the corresponding rays of the two arcs, their effects will mutually destroy each other.

We may therefore suppose all the rays sent by the different parts of the undulation at AE, to the point P, to be of equal intensity, since the only rays, with respect to which this hypothesis would be incorrect, are such as have no sensible influence on the quantity of light which it receives. For the same reason, in order to simplify the calculation of the result of all these elementary undulations, we may consider their constituent motions as performed in the same direction, the angles which they form with each other being inconsiderable. The problem is thus reduced to that which has been solved in the Memoir on Diffraction, already quoted: *To find the result of any number of systems of parallel undulations of light, of the same frequency, when their intensities and relative situations are given.*—The intensities are here proportional to the length of the small illuminating arcs, and the relative situations are given from the differences of the paths described.

We have considered, correctly speaking, only the section of the undulation made by a plane perpendicular to the margin of the screen represented by A. We may now take into account the whole extent of the undulation, and suppose it to be divided, by equidistant meridians perpendicular to the plane of the figure, into infinitely thin wedges or strata; and we may apply to all of these the reasoning which has

been employed for one section, and thus demonstrate that the rays which have a marked obliquity must destroy each other.

These strata, in the case here considered, being all parallel to the edge of the screen, and infinitely extended, while the undulation is intercepted but on one side; the intensity of the result of all the impressions, which they transmit to P, will be the same for each of them: for the rays emanating from them must be considered as of equal intensity, at least for the very small extent of the generating undulation, which has a sensible influence on the light received at P. Besides, each elementary result will evidently be retarded by the same quantity, with respect to the ray derived from the point of the stratum nearest to P, that is to say, to the point in which it cuts the plane of the figure: consequently the intervals between these elementary results will be equal to the differences of the paths described by the rays AP, m'P, mP, and so forth, which are in the plane of the figure, and their intensities will be proportional to the arcs Am', m', m, mM, and so forth. We may therefore consider the intensity of the general result as determined by the calculation already mentioned, as belonging to the section of the undulation made by a plane perpendicular to the margin of the screen.

While the outline of the screen remains rectilinear, it is sufficient, in order to determine the situations of the dark and light stripes, and their relative intensities, to consider the section of the undulation made by a plane perpendicular to that outline: but when it is curved, or composed of lines meeting at any angles, it becomes necessary to obtain the integral effect for two directions at right angles to each other, or for a circle surrounding the point considered. This last method is the most simple in some particular cases, as when we have to calculate, for example, the intensity of the light in the projection of the centre of a circular screen or opening: [a simplification which, though sufficiently obvious, had perhaps not occurred to Mr. Fresnel, until it was pointed out to him by the Translator of this paper.]

It will now be easy to form a distinct idea of the method

which must be followed, in order to calculate the situation and the intensity of the dark and bright stripes, in the different circumstances under which it is proposed to compare the theory with experiment. When the screen is infinitely extended on one side, or is broad enough to allow us to neglect the rays which pass beyond it, we are to determine, for any point P at the distance of the place at which the fringes are to be observed, the result of all the elementary undulations coming from the part AMF only of the incident wave; and comparing the intensities at different collateral points, P, P′, P″, we are to find the situation of the darkest and the brightest points. In this manner we find, for a screen closed on one side, 1st, that the intensity of the light decreases rapidly within the [shadow] beginning from the tangent CAB, and *so much the more rapidly as the undulation is smaller*; and this in a continuous manner, without any alternations of maxima and minima; 2ndly, that out of the shadow, the intensity of the light, after augmenting considerably to a certain point, which may be called a maximum of the first order, decreases to another point, which is the minimum of the first order: that it increases again to a second maximum, to which succeeds a second minimum, and so forth; 3rdly, that none of these minimums completely vanish, as in the case of fringes produced by the concourse of two luminous pencils of equal intensity, and that the difference between the maxima and minima diminishes in proportion as we go further from the shadow; whence we may understand why the fringes which surround shadows in a homogeneous light, are less marked and less numerous, than those which are obtained by a combination of two mirrors, and those in white light much less brilliant; 4thly, that the intervals been the maxima and minima are unequal, and diminish, as we depart from the shadow, in proportions which remain unaltered, whatever may be the distance from the screen at which we measure them; and 5thly, that the same maxima and minima, calculated for different distances from the screen, are situated in hyperbolas of a sensible curvature, of which the foci are the edge of the screen, and the luminous point. All these consequences of the theory are precisely confirmed by experiment.

The general formula gives the position of the maxima and minima for any distances whatever of the luminous point from the screen, and from the screen to the micrometer, when the length of the undulation of the light employed is known. In order to submit the theory to a decisive test, instead of determining the length of the undulation by measures of the external fringes, and then employing it in calculations of the same kind, I deduced it from an experiment on diffraction of a very different kind; and after having first verified it by the fringes obtained from two mirrors, of which it represented the breadth within a hundredth part of the truth, I introduced it into the formula which I afterwards compared with 125 measurements of exterior fringes, made under very different circumstances; for the distance of the radiant point from the screen was varied from four inches to six or seven yards, and the distance between the screen and the micrometer was varied from $\frac{1}{13}$th of an inch to more than four yards: and the results of all these comparisons were perfectly satisfactory, as may be seen in the comparative table published in the XIth volume of the Annales de Chimie et de Physique, p. 339, 343.

When the screen, instead of extending infinitely on one side, is narrow enough to admit some light on that side, not too much weakened by the rapid decrease of intensity produced by obliquity, we must take into the calculation the light on both sides, and find, for each point of the shadow, the general result of all the elementary undulations derived from the points on the right and left. We thus demonstrate that the interior parts of the shadow must be divided by a series of dark and bright stripes, nearly equal in breadth, of which the situations differ very little from those which would be deduced from the approximative formula which has already been given for the same purpose, when they are still separated from the borders of the shadow by an interval of several of their breadths. But when the opaque body is narrow enough, and the micrometer far enough removed for the observed stripes to be very near the exterior stripes, then the results of this more exact calculation, as well as those of experiment, show that the approximation is no longer accurate. The cal-

culation determines also, with remarkable precision, the singular alterations which the exterior fringes often undergo, when the other series extends beyond the shadow, and mixes its effects with those of the exterior.

I have also verified the theory by examining the fringes derived from a narrow slit of indefinite length; and determining, for the different points enlightened by the luminous pencil, the result of all the elementary undulations derived from the part of the primitive wave comprehended in the breadth of the slit; and I have found a satisfactory agreement between the calculation and the observations, even when the fringes thus obtained afforded the most capricious and apparently irregular appearances.

In this mode of considering the problems relating to diffraction, we have not taken into the calculation the greater or less thickness of the edges of the screen, but merely the extent of the primitive wave which is capable of sending elementary undulations to the points for which we are to find the intensity of illumination; and the opaque substance has no other effect than simply to intercept a part of the wave: for this reason the result is necessarily independent of the nature of the body, of its mass, and of the thickness of its edges. Nevertheless, if the surface of the edges were very extensive, it would be impossible to consider the portion of the wave as quitting the slit without having received some previous modification, and it would be necessary to take into the calculation the small fringes derived from the effect of the remoter parts of the slit. But while the thickness is moderate, or the edges rounded off into a well marked curve, the small fringes derived from this cause may be neglected, and the emerging wave may be considered as of equal intensity throughout, at the moment of its quitting the screen, especially if the intensity of the light is to be calculated for a pretty considerable distance from the screen. We must not, indeed, forget, that according to the reasoning which has been employed, the formulas for diffraction are only sufficiently exact when this distance is very considerable, in comparison with the breadth of an undulation, since it is in this case only that we can neglect the rays that are decidedly oblique, and

can suppose all those, which are essentially concerned in the effect, to be nearly of equal intensity. It is not, however, surprising that the same formulas will give the position of the fringes with sufficient accuracy at small distances from the screen, when its edges are thin, since, the mean breadth of an undulation being but about one fifty thousandth of an inch, a tenth of an inch becomes comparatively a very considerable distance.

These are the three principal kinds of phenomena presented to us by diffraction, when the edges of the screen, or of the opening made in it, are sufficiently extensive to afford fringes independent of any effect from their terminations: and in such cases it is sufficient to make the integral calculation for the plane perpendicular to the edges of the screen only, in order to determine the position of the dark and bright stripes, and their comparative intensities. But when the screen or the opening are of small dimensions in every direction, it becomes necessary to extend the integration to the effects produced in two perpendicular planes: and the results of the calculation agree perfectly with observation, as will appear from two curious instances.

When the screen is circular, the calculation leads to this singular result, that the centre of the shadow projected by it must be as much enlightened as if the screen were not in existence. It was Mr. Poisson that first pointed out this consequence of my formulas, which I did not at first observe, though it is immediately deducible from the theory by very simple geometrical considerations. Mr. Arago made the experiment with the shadow of a screen $\frac{1}{13}$th of an inch in diameter, perfectly round, and fixed on a plate of glass. The result confirmed the fact which had been announced by the theory. It is only the centre itself that possesses this property, and the same brightness is only extended to a sensible distance from this mathematical point when the screen is of very small diameter, and when its shadow is observed at a great distance: for the wider that the screen becomes, the more the little bright circle is contracted; and when the screen is four tenths of an inch in diameter, we only see a single point of light, at the distance of a yard, even with a powerful magnifier. It must be observed, that if the screen

were too large, the reasoning, from which the formulas have been deduced, would no longer be rigorously applicable to the rays inflected into the shadow, because of their too great obliquity, which would render it impossible to consider their effects as equal in intensity to those of the direct rays.

When we calculate, by the same formulas, the intensity of the light in the centre of the projection of a small circular aperture, made in a large screen, we find that this centre will exhibit alternately a bright and a dark appearance, according to the distance at which the shadow is viewed; and that in homogeneous light this darkness must be perfect. This new inference from the general formulas may be deduced from the theory by very simple geometrical considerations. Thus we find that the values of the successive distances, at which the centre of the shadow becomes completly dark, are $b = \frac{ar^2}{2ad - r^2}$, $b = \frac{ar^2}{4ad - r^2}$, $b = \frac{ar^2}{8ad - r^2}$; and so forth; r being the semidiameter of the aperture, a and b its respective distances from the luminous point and from the micrometer, and d the length of the undulation of the light employed. Now, if we place the micrometer at the distances indicated by these formulas, we observe, in fact, that the centre of the projection of the opening is so completely deprived of light, that it appears like a spot of ink in the middle of the illuminated part, at least with respect to the minimums of the first three orders, as indicated by the formulas here inserted: those of the subsequent orders, which are nearer to the screen, exhibiting no longer the same degree of darkness, on account of the want of homogeneity of the light employed.

There is still a multitude of other phenomena of diffraction, such as those of multiplied and coloured images, reflected by striated surfaces, as seen through a texture of fine fibres, as well as the coloured rings, produced by an irregular collection of such fibres, or of light powders, consisting of particles nearly equal, placed between the eye of the spectator and a luminous object; all of which may be explained and rigorously computed by means of the theory which has been laid down. It would, however, occupy too much of our time to describe them here, and to

show how exactly they concur in confirming the theory; which indeed appears to be abundantly demonstrated by the numerous and diversified facts which have been already adduced in support of it. It will be sufficient to conclude this extract of the Memoir on Diffraction with a detailed description of an important experiment of Mr. Arago, which furnishes us with a method of determining the slightest differences of the refractive powers of bodies, with a degree of accuracy almost unlimited.

We have seen that the fringes, produced by two very narrow slits, are always placed symmetrically with regard to a plane passing through the luminous point and the middle of the interval between the slits, as long as the two pencils of light which interfere have passed through the same medium, for instance, the air, as happens in the ordinary arrangement of the apparatus. But the result is different when one of the pencils continues to pass through the air, and the other has to be transmitted by a more refractive body, a thin plate of mica, for example, or a piece of glass blown very thin: the fringes are then displaced, and carried towards the side on which the transparent substance is placed: and if its thickness becomes at all considerable, they are removed out of the enlightened space, and disappear altogether. This important experiment, which was first made by Mr. Arago, may also be performed with the apparatus of the two mirrors, if the plate be placed in the way of one of the pencils, either before or after its reflection.

Let us now see what inference may be drawn from this remarkable fact, by the assistance of the principle of interferences. The light stripe in the middle is always derived, as we have already seen, from the simultaneous arrival of rays which have issued at the same moment from the luminous point; consequently, in the common circumstances of the experiment, they must have described paths exactly equal, in order to arrive in the same time at the place of meeting: but it is obvious that if they pass through mediums in which light is not propagated with the same velocity, that pencil, which has travelled the more slowly, will arrive at the given point later than the other, and the point will

therefore no longer be in the bright stripe. The stripe must therefore necessarily change its place towards the pencil which travels the more slowly, in order that the shortness of its path may compensate for the delay during its transmission through the solid: and the converse of the proposition enables us to conclude, that where the stripes are displaced, the pencil towards which they move has been retarded in its passage. The natural inference, therefore, "from Mr. ARAGO's experiment," is, that light is propagated more rapidly in the air than in mica or glass, and generally in all bodies more refractive than the air; a result directly opposite to the Newtonian theory of refraction, which supposes the particles of light to be strongly attracted by dense substances, which would cause the velocity of light to be greater in these bodies than in rarer mediums.

This experiment furnishes a method of comparing the velocity of the propagation of light in different mediums, [or, in other words, the refractive density, which is always supposed in this theory, to be reciprocally proportional to it.] If, in fact, we measure very accurately, by means of a spherometer, the thickness of the thin plate of glass which has been placed in the way of one of the luminous pencils, and if the displacement of the fringes has been measured by the micrometer; since we know that, before the interposition of the glass, the paths described were equal for the middle of the central stripe, we may calculate how much difference is occasioned by the change of position, and this difference will give the retardation in the plate of glass, of which the thickness is known: so that, by adding this thickness to the difference calculated, we shall find the little path which the other pencil has described in the air, while the former was transmitted by the plate of glass; and this path, compared with the thickness of the plate of glass, will give the proportion of the velocity of the light in the air, to its velocity within the glass.

We may also consider this problem in another point of view, with which it is convenient to make ourselves familiar. The duration of each undulation, as we have seen, does not depend on the greater or less velocity with which the agita-

tion is propagated along the fluid, but merely on the duration of the previous oscillation which gave it birth; consequently, when the luminous waves pass from one medium into another, in which they are propagated more slowly, each undulation is performed in the same interval of time as before, and the greater density of the medium has no other effect than that of diminishing the length of the undulation, in the same proportion as the velocity of light is diminished: for the length of the undulation is equal to the space that the first agitation describes during the time of a complete oscillation. We may therefore calculate the relative velocities of light in different mediums, by comparing the length of the undulations of the same kind of light in those mediums. Now, the middle of the central stripe is formed by the reunion of such rays of the two pencils as have performed the same number of undulations, in their way from the luminous point, whatever may be the nature of the mediums transmitting the light. If then the central stripe is brought towards the side of the pencil which has passed through the glass, it is because the undulations of light are shorter within the glass than in the air; and it is necessary, in consequence, that the path described on this side should be shorter than the other, in order that the number of undulations may remain the same. Let us suppose, then, that the central stripe has been displaced to the extent of twenty breadths of fringes, for example, or of twenty times the interval between the middle points of two consecutive dark stripes; we must necessarily conclude that the interposition of the plate of glass has retarded the progress of the pencil passing through it to the extent of twenty undulations; or that it has performed within the plate twenty undulations more than the same pencil would have performed in an equal thickness of air, since each breadth of a fringe answers to the difference of a single undulation. If then we know the thickness of the plate, and the length of an undulation of the light employed, which is easily deduced from the measurement of the fringes, by the formula that has been given, we can calculate the number of undulations comprehended in the same thickness of air, and by adding twenty to the number, we shall have that of the un-

dulations performed in the thickness of the glass; and the proportion of these two numbers will be that of the velocities of light in the different mediums. Now this proportion is found by experiment the same with that of the sines of incidence and of refraction between air and glass; which agrees with the theory of the refraction of undulations, as will be seen hereafter.

The same experiment may be employed, on the other hand, for determining with extreme precision the thickness of a thin plate of a substance of known refractive density; placing it in the way of one of the two pencils of light, and measuring the displacement of the fringes which it occasions.

This method of determining refractive densities is however liable to some difficulties, when we wish to apply it to a body much more dense than air, such as water, or glass, for example; since it is necessary to employ a very thin plate only, in order that the fringes may not be too much displaced for observation; and then it becomes difficult to measure the thickness of such a plate with sufficient accuracy. We may, indeed, place in the way of the other pencil a thick plate of a transparent substance, of which the refractive density has been ascertained by the ordinary methods, and we can then employ as thick a plate of the new substance. But then it becomes simpler to measure its refractive density by the common method: [unless we choose to immerse the whole apparatus in a fluid very nearly approaching to it in refractive density, which may sometimes be done without inconvenience. Tr.]

The case, in which Mr. Arago's experiment has a decided advantage over the direct method, is when we desire to determine very slight differences of velocity in mediums of nearly equal refractive density: for by lengthening the passage of the light in the two mediums of which we wish to compare the refractive density, we can increase the accuracy of the results almost without limit. In order to form an idea of the extreme precision that may be attained by these measurements, it is sufficient to observe that the length of the yellow undulations in air being about .000021 E.I., there are two millions of them in the length of about 42 inches. Now

it is very easy to observe the difference of one fifth of a fringe, which corresponds to a retardation of one fifth of an undulation in one of the pencils, that is, the ten millionth part of the whole length of 42 inches; we might therefore, by introducing any gas or vapour into a tube of this length, terminated by two plane glasses, estimate very accurately the variation of its refractive power.

I take the length of an undulation of the yellow rays, which are the most brilliant of the spectrum, and of which the dark and light stripes consequently coincide with the darkest and brightest stripes of the fringes produced by white light, which is commonly employed in these experiments, both because of its greater brightness, and because of the more marked character which it gives to the central stripe, so as to prevent any other from being mistaken for it.

It was an apparatus of this kind that Mr. Arago and myself employed for measuring the difference of the refractive powers of dry air, and of air saturated with moisture at 80° F., which is so small, that it would escape every other method of observation, because the greater refractive power of aqueous vapour is almost exactly compensated by the less specific gravity of moist air. But, in the generality of cases, the slightest mixture of one vapour or gas with another produces a considerable displacement in the fringes: and if we had a series of experiments of this kind, made with care, the apparatus might become a valuable instrument of chemical analysis.

[To be continued.]

iii. *Remarks on the Action of* Corpuscular Forces. *In a Letter to* Mr. Poisson.

My dear Sir,

I am very glad to see that you have been applying your analytical powers to the investigation of the acoustical effects of corpuscular forces, and that, among many more refined determinations, you have confirmed several of the results relating to sounding bodies, which were published twenty years ago in my Lectures on Natural Philosophy: though they were generally such as might have been derived from the calculations of Bernoulli and Euler; which I attempted in some

measure to simplify by the introduction of the element which I called the *Modulus of Elasticity* of each substance. You have very properly observed that it is often difficult to represent the combination of these corpuscular forces by an integral, since in many practical cases the integral must vanish, where it would naturally be applied to the phenomena: and, from similar considerations, I trust you will be prepared to admit the objections that I made long ago, to the reasoning of your great predecessor, Mr. Laplace, to whose station in the mathematical world you appear so eminently qualified to succeed.

The equation, which may be called final, in Mr. Laplace's Supplement to the Xth Book, p. 47, is $Q \cos. (\varpi - \theta) = (2\varrho - \varrho') K \sin. \theta$. Now this, in my opinion, is a perfect *reductio ad absurdum*: for Q must *always* be *incomparably* less than K; the attraction of the particles lying between a cylinder and its tangent plane being *always* infinitely less than that of the particles in an angular or prismatic edge: or if this were denied in general, it would obviously become true when the cylinder itself becomes a plane, and Q vanishes altogether; which will always be the state of the problem, when the surface of the solid is so inclined to the horizon, that the surface of the fluid may remain horizontal, the appropriate angle of contact being unaltered in these circumstances, as it is easy to show by making the experiment with mercury.

I entreat you to consider this objection with patient attention, and to tell me if you can find any arguments to supersede it. I would also presume to ask your opinion of my own method of deducing the force of capillarity from the elementary attractions and repulsions of bodies, at the end of my Illustrations of the Celestial Mechanics, Art. 382; Appendix A, p. 329 to 337. The volume is in the Library of the Academy; or I should have taken the liberty of sending you a copy, as an inadequate return for so many valuable communications with which you have had the kindness to favour me.

Believe me always, dear Sir,

Very truly yours,

London, 18 *Nov.* 1827. * * * *

iv. *Calculations of* Lunar Phenomena. *By* Thomas Henderson, Esq.

Principal Lunar Occultations of the Fixed Stars in the Months of January, February, March, and April, 1828; calculated for the Royal Observatory at Greenwich.

Date.	Names of Stars.	Magnitude.	Immersion and Emersion. Mean Time.	Apparent Difference of Declination.	Point of Moon's Limb.
			H. M. S.	′ ″	°
Jan. 4	κ Cancri	5.6	Imm. 10 51 43	13 18 S.	172 R.
			Em. 11 44 48	7 19 S.	91 R.
31	α^1 Cancri	6	Imm. 11 14 52	7 45 S.	134 L.
			Em. 12 35 56	2 19 N.	88 R.
,,	κ Cancri	5.6	Imm. 18 38 0	0 58 N.	47 L.
			Em. Under	Horizon.	
Feb. 7	α^2 Libræ	3	Imm. 20 29 54	1 35 S.	69 L.
			Em. 21 37 51	4 33 N.	107 R.
22	δ^3 Tauri	5	Imm. 7 0 9	3 47 S.	90 L.
			Em. 8 16 39	6 34 S.	146 R.
28	ω Leonis	6.7	Imm. 11 24 25	14 57 S.	165 L.
			Em. 12 3 15	9 17 S.	145 R.
March 10	ϱ^1 Sagittarii	5	Imm. 16 14 54	4 24 N.	105 L.
			Em. 17 20 39	1 25 N.	62 R.
23	υ Geminorum	5.6	Imm. 8 4 36	2 48 N.	55 L.
			Em. 9 18 11	7 46 N.	94 R
24	*k* Geminorum	5	Imm. 9 12 5	3 53 S.	78 L.
			Em. 10 28 34	3 55 N.	111 R.
26	κ Cancri	5.6	Imm. 7 41 25	7 6 S.	132 L.
			Em. 9 3 38	3 14 N.	83 R.
April 2	ν^1 Libræ	6	Imm. 14 7 43	12 43 N.	38 L.
			Em. 14 34 56	15 49 N.	10 R.
	ν^2 Libræ	6.7	Imm. 13 58 36	2 1 S.	99 L.
			Em. 15 13 58	6 19 N.	76 R.
29	α^1 Libræ	6	Imm. 16 15 38	14 48 S.	126 L.
			Em. 16 48 16	11 55 S.	174 R.
	α^2 Libræ	3	Imm. 16 33 5	15 54 S.	145 L.
			Em. 16 43 5	15 3 S.	162 L.

The fifth column shows the apparent difference of declination between the Star and Moon's centre at the immersion and emersion; the letters N and S denoting the Star to be north or south from the Moon. The sixth or last column shows the point of the Moon's limb where the immersion and emersion take place, reckoning from the vertex or highest point; the letters L and R signifying to the left hand or right hand of the observer.

An error of 11 seconds in the computed difference of declination between the Moon and Star, will be sufficient to convert the expected Occultation of α^2 Libræ, on 29th April, into an Appulse; and a less error will considerably affect the times and places of immersion and emersion.

[To be continued.]

ELEMENTS for computing the ECLIPSES of the SUN and OCCULTATIONS of the PLANETS by the MOON, in the Year 1828.

	Conjunction in A. R. Apparent Time.	Diff. Dec.	Relative H. M.	Relative Orb. Ang.	☉ or Planet's A. R. at ☌	☉ or Planet's N. P. D. at ☌	Nearest Approach	Time of nearest Approach, Apparent Time.	☉ or Planet's Horary Motion in A. R. in Time	☉ or Planet's Horary Motion in N. P. D.	☉ or Planet's Semi-diameter	☉ or Planet's Hor. Par.
☽	D. H. M. S.	☽ ′ ″	′ ″	° ′	H. M. S.	° ′ ″	′ ″	D. H. M. S.	SEC.	″	″	″
♃ Jan.	11 10 47 11	21 1 S.	34 6	S. 76 58 E.	14 35 47	104 2 30	20 28	11 10 38 51	+ 1·3	+ 6	17	2
♂ Jan.	11 16 40 36	4 29 N.	33 14	S. 78 4	14 49 29	105 12 34	4 23	11 16 42 16	+ 5·9	+ 27	3	5
♃ Feb.	7 22 17 54	5 44 N.	33 26	S. 77 35	14 46 40	104 48 7	5 36	7 22 20 7	+ 0·6	+ 2	18	2
♃ Mar.	6 4 48 7	16 46 N.	33 29	S. 77 47	14 49 7	104 53 57	16 23	6 4 54 29	− 0·2	− 1	20	2
♃ April	2 8 6 49	9 6 N.	34 12	S. 77 26	14 42 44	104 21 7	8 53	2 8 10 18	− 0·9	− 4	21	2
☉ April	13 21 23 53	8 50 N.	31 23	N. 74 7	1 30 20	80 32 14	8 30	13 21 19 16	+ 9·2	− 54	958	9
♃ April	29 10 49 53	9 3 S.	34 50	S. 76 40	14 30 21	103 22 5	8 48	29 10 46 17	− 1·2	− 6	22	2
☿ May	12 8 58 41	7 44 N.	27 31	N. 78 28	2 30 50	76 35 6	7 35	12 8 55 19	+19·8	−119	3	7
♃ May	26 14 58 46	20 58 S.	34 38	S. 75 54	14 17 53	102 23 57	20 20	26 14 49 55	− 0·9	− 4	21	2
♃ June	22 21 27 33	14 27 S.	33 40	S. 75 26	14 11 5	101 55 51	13 59	22 21 21 5	− 0·3	− 1	20	2
♀ July	13 12 0 30	68 43 S.	30 15	S. 76 50	8 55 0	76 17 27	66 55	13 11 29 28	− 3·4	+ 22	26	27
♃ July	20 6 18 33	10 12 N.	32 33	S. 75 28	14 12 35	102 11 28	9 53	20 6 23 16	+ 0·5	+ 3	18	2
♃ Aug.	16 17 18 6	44 49 N.	31 50	S. 75 59	14 22 10	103 7 21	43 29	16 17 38 31	+ 1·2	+ 6	17	2
♀ Sept.	5 3 7 12	4 10 S.	28 8	S. 78 3	8 13 4	75 7 21	4 5	5 3 5 22	+ 5·8	− 1	18	·19
☉ Oct.	8 12 23 35	6 39 S.	29 6	S. 73 2	12 57 44	96 10 27	6 21	8 12 19 35	+ 9·2	+ 57	·963	9
♀ Dec.	3 13 30 46	39 10 S.	29 39	S. 75 23	14 3 58	100 19 38	37 54	3 13 10 46	+11·5	+ 61	7	8·

The places of the Sun and Moon have been taken from the Nautical Almanac, those of Mercury from Lindenau's Tables, and those of the other Planets from Schumacher's Ephemeris.——The sign + denotes the motion in A. R. to be direct; the sign —, retrograde. The sign + denotes the motion in N. P. D. to be towards the South; the sign — towards the North.——None of the preceding Conjunctions will prove to be an Eclipse or Occultation visible at Greenwich.

Apparent Distance of Jupiter's Satellites from Jupiter's Centre, at his Conjunctions in A. R. with the Moon.

Date.	Satellite.	Distance.	Date.	Satellite.	Distance.
1828.			1828.		
January 11	I.	1′ 14″ East	May 26	I.	1′ 44″ East
	II.	0 51 ——		II.	3 8 West
	III.	4 16 West		III.	5 15 ——
	IV.	2 21 ——		IV.	6 38 East
February 7	I.	1 6 West	June 22	I.	1 59 West
	II.	2 40 East		II.	0 4 East ‡
	III.	2 16 West		III.	3 10 West
	IV.	4 27 ——		IV.	8 56 ——
March 6	I.	1 51 East	July 20	I.	1 51 East
	II.	2 24 West		II.	2 50 ——
	III.	3 14 East		III.	1 55 ——
	IV.	8 44 ——		IV.	4 51 ——
April 2	I.	1 44 West	August 16	I.	1 43 West
	II.	0 15 —— *		II.	1 12 ——
	III.	4 58 East		III.	4 21 East
	IV.	7 30 West		IV.	1 53 ——
29	I.	0 14 West †			
	II.	2 51 East			
	III.	0 32 West			
	IV.	1 6 East			

* On Jupiter's disk. † Eclipsed. ‡ On Jupiter's disk.

These Configurations have been computed from De Lambre's Tables.

MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *On the Adhesion of Screws.*—The following results, respecting the force necessary to draw iron screws out of given depths of wood, are by Mr. Bevan, and should be placed by the side of those he has given with regard to nails*.

"The screws I used were about two inches in length, 0.22 diameter at the exterior of the threads, 0.15 diameter at the bottom, the depth of the worm or thread being 0.035, and the number of threads in one inch =12. They were passed through pieces of wood exactly half an inch in thickness, and drawn out by the weights specified in the following table:

Dry beech..............	460 pounds
Do. Do.................	790
Dry sound ash...........	790
Dry oak.................	760
Dry mahogany...........	770
Dry elm.................	655
Dry sycamore............	830

"The weights were supported about two minutes before the screws were extracted.

"I have also found the force required to draw similar screws out of deal and the softer woods about half the above.

"From which we may infer as a rule to estimate the *full* force of adhesion, in hard wood......200.000 $d\,\delta\,t = f$,

and in soft wood......100.000 $d\,\delta\,t = f$,

d being the diameter of the screw; δ the depth of the worm or thread; and t the thickness of the wood into which it is forced;—all in inches; f being the force in pounds to extract the same." We may, from the above experiments, observe the approximation to perfection in the art of screw making; for had the screw been greater in diameter, there would have been a waste of material, or had it been less, it would not have been sufficiently strong, which may be proved as follows: the cohesion of wrought iron has been found, from a number of experiments, to be about 43000lbs. per cylindrical inch; and as the smallest diameter of screw used in my experiment was 0.15, it would have been torn asunder by a force of about 968lbs.; or if the hard wood had been about $\frac{5}{8}$ of an inch thick into which it had been screwed, the screw would have been broken instead of forcing its passage out of the wood.—*Phil. Mag. N. S.* ii. 291.

2. *Improvement in Steam-engines.*—According to the valuable records kept of the duty of the steam-engines at the mines in Cornwall, a most important improvement has been effected in two

* See page 360, vol. xvii. of the former series of this Journal.

instances, of engines erected by Captain Samuel Grose; dependent entirely upon attention to the smaller details of the machines. The best engines, heretofore, had not done more than raise forty millions of pounds of water one foot high, by each bushel of coals consumed, except indeed upon short occasions. In one of the cases in question, an engine at Wheal Hope, of sixty-inch cylinder, working single as usual, the duty rose to fifty, fifty-four, and fifty-five millions of pounds; and in the other, an engine of eighty-inch cylinder, at Wheal Towan, the duty rose in

April.................	61,877,545
May..................	60,632,179
June.................	61,762,210
July.................	62,220,820
August...............	61,764,166

thus exceeding by nearly fifty per cent. what had been effected before that time.

3. *Improved Clock.*—Among the articles displayed at the first National Exhibition of the Objects of Arts and Industry, at Neufchatel, Switzerland, last year, was a clock made by F. Houriet, of Locle; in which steel was used only in the main springs and in the axes of the moveable parts; all the other parts were in brass, gold alloy, and white gold. The number of pieces in gold, gold and silver, gold and platina, is sixty-two: all the pivots turn on jewels, and the functions of the free escapements are effected also by means of pallets in precious stones. It had been supposed that the escapements and the spiral spring not being of steel, inconvenience would result from the smaller degree of elasticity, but numerous trials with favourable results have removed the objection; and it appears that gold, hardened either by hammering or other means, is more elastic than hardened and untempered steel. The clock had gone for six days, exposed to the contact of a magnet competent to lift twenty-five or thirty pounds, without suffering any derangement.—*Rév. Ency.*

4. *Method of dividing Glass by Friction.*—The following method is described by Dr. Hare: "Some years ago Mr. Lukin showed me that a small phial or tube might be separated into two parts, if subjected to cold water after being heated by the friction of a cord made to circulate about it, by two persons alternately pulling in opposite directions. I was subsequently enabled to employ this process in dividing large vessels of four or five inches in diameter, and likewise to render it in every case more easy and certain by means of a piece of plank forked like a boot-jack, and also having a kerf cut by a saw, parallel to and nearly equidistant from the principal surfaces of the plank, and at right angles to the incisions productive of the fork.

"By means of the fork, the glass is easily held steadily by the hand of one operator; by means of the kerf, the string, while circu-

lating about the glass, is confined to the part where the separation is desired. As soon as the cord smokes, the glass is plunged in water, or if too large to be easily immersed, the water must be thrown upon it; the latter method is always preferable when, upon immersing the body, the water can reach the inner surface. As plunging is the most effectual method of employing the water in the case of a tube, I usually close the end which is to be immersed."—*Silliman's Journal*, xiii. 7.

5. *Use of Soapstone in diminishing Friction.* —In a letter to Professor Silliman upon this subject, Mr. E. Bailey of Boston, says, " I understand the Soapstone has been used for this purpose in the extensive manufactories at Lowell, for about two years, and with great profit and success. Besides answering the purpose to which it is applied very much better than any other substance that can be procured, it saves a great deal of trouble and expense. It is first thoroughly pulverized, and then mixed with oil, tallow, lard, or tar, whichever may be the best adapted to the use for which it is designed. It is of course important to procure that which is free from *grit*, and it can be purified in a good degree by mixing the powder with oil, and decanting it after it has stood a few minutes. The heavier particles will form a sediment to be rejected. It is used in all kinds of machinery where it is necessary to apply any unctuous substance to diminish friction, and it is said to be an excellent substitute for the usual composition applied to carriage-wheels.

Some idea of the value of soapstone thus applied, may be formed from the following fact communicated by D. Moody, Esq., the superintendent of the tar-works on the mill-dam near this city. Connected with the rolling machine of that establishment, there is a horizontal balance-wheel, weighing *fourteen tons*, which runs on a step of five inches diameter, and makes from seventy-five to one hundred revolutions in a minute. About one hundred tons of iron are rolled in this machine in a month; yet the wheel has sometimes been used from three to five weeks without inconvenience, before the soapstone has been renewed. The superintendent thinks, however, that it ought to be more frequently employed.

" The use of soapstone was discovered at Lowell. It has been said never to fail in producing the desired result when applied to machinery which had began to be heated, even in those cases when nothing else could be found that would answer the purpose."—*Silliman's Journal*, xiii. 192.

6. *On peculiar Physical Repulsions, by* M. Saigey.—I intend to give in this bulletin the description of a very simple apparatus, by means of which I have made many experiments, which have conducted me to the following results:—

i. All bodies exert between themselves a feeble repulsive action in ordinary circumstances. The repulsion between bismuth and

antimony and the poles of a magnetic needle, is a case of this general law, and is not due to magnetism. Nor is it magnetism which occasions the direction of needles formed of other substances than iron, announced lately by M. Becquerel.

ii. A very marked attraction may be observed between a cold and a heated body, or between two bodies of different temperature, whether screens be interposed or not.

iii. The metallic plates in the Cabinet de Physique de Paris, intended for the repetition of M. Arago's experiments on magnetism by rotation, contain more or less of iron capable of attracting a very mobile magnetic needle. These plates, and those of M. Arago, were made by the same person and from the same materials.

iv. I believe that, in many cases, results obtained without the appreciable developement of magnetism or electricity, have been attributed to these powers; and from well-proved experiments I shall deduce new results relative to the diurnal variation of the needle, the direction of the plumb-line and the density, temperature and attraction of the planetary masses.—*Bull. Univ.* A. viii. 287.

7. *On the Magnetic Effects of Metals in Motion.*—M. Seebeck has endeavoured to determine the effects of various metals in diminishing the oscillations of a magnetic needle $2\frac{1}{3}$ inches in length, and suspended by a silk fibre three lines distant from and above the plates. The oscillations were counted from an amplitude of 45° to 10°.

116 oscillations above a plate of marble			
112	layer of mercury	2	lines in thickness.
106	plate of bismuth	2	,,
94	platina	0.4	,,
90	antimony	2.0	,,
89	lead	0.75	,,
89	gold	0.2	,,
71	zinc	0.5	,,
68	tin	1.0	,,
62	brass	2.0	,,
62	copper	0.3	,,
55	silver	0.3	,,
6	iron	0.4	,,

It is also stated that he has found, from experiments, that by alloying such metals as are magnetic, like iron, nickel, and cobalt, with other metals, which like antimony diminish the magnetic force, alloys are obtained entirely neutral in their effects; thus the alloys formed by four of antimony with one of iron, three of copper with one of antimony, and two of copper with one of nickel, produce no diminution of the number of oscillations, these amounting to 116 as with the plate of marble. These three alloys are, therefore, the best for the manufacture of compasses, those of copper and nickel being the most malleable.—*Annal. des Phy.* 1826. *Bull. Univ.* A. viii. 136.

8. *Duration of the Effects of Light upon the Eye.*—M. Plateau of Liege has endeavoured to determine the length of time during which the impression of certain luminous rays upon the eyes remains; and has given the following results:

Flame	0″.242
Ignited Charcoal	0″.229
White	0″.182
Blue	0″.186
Yellow	0″.173
Red	0″.184

9. *On the Measurement of the Intensity of Light, by* M. Peclet.—A very usual photometrical process is to interpose an opaque body between a white screen and the two lights to be measured, and to move the latter until the shadows produced are of equal intensity; the intensity of the lights being then as the square of their distances from the shadows they illuminate. Sometimes a translucent body, as unpolished glass or oiled paper, is used in place of an opaque one, the shades produced by transmission being observed.

In both these methods, the apparent intensity of the shadow varies with the position of the observer. If the shadows are equal when observed from a point perpendicular to the white screen at the middle of the distance of the two shades, they will be no longer so on removing from that position, and the shadow nearest to the observer will always appear the darkest. These apparent variations are greater as the shadows are farther apart, or with reflected shadows as the screen is smoother, or with transmitted shadows as the interposed obstacle is more diaphanous.

The explanation given of this fact is, that unpolished opaque bodies, like paper, plaster, &c. never disperse the light incident upon them, in an uniform manner, more rays passing in the direction in which regular reflexion would take place, than in any other. Hence, when two equal shadows are produced upon such a surface, either by two equal lights at equal distances, or by two unequal lights at unequal distances; the shadow nearest to the observer must necessarily appear deeper than the other, because it is enlightened by the nearest light, the rays from which are reflected in greatest abundance away from the observer; and, on the contrary, the shadow further from the observer should appear lightest, because the rays which fall on it from the furthest light are reflected in greatest abundance towards the side on which the observer stands. The reason, also, why the effect is greater as the shadows are further apart is evident; and why in every case it is reduced to nothing when the observer is in a plane perpendicular to the screen and equidistant from the two shadows.

From these facts and explanations it may be concluded, that, in all photometrical measurements by reflected shadows, the screens should have all smoothness removed from them, and the two sha-

dows brought as near together as possible, and even made to touch or over-lap; or that, when this cannot be done, the observation should be made from a point equidistant from the two shadows. As to the shadows by transmission, the apparent variations of intensity are so great for small changes in the position of the eye, as to render the method altogether inapplicable.—*Bull. Univ.* A. viii. 248.

10. *On the apparent Decomposition of White Light by a Reflecting Body when in Motion.*—The following experiment is described in the MSS. of M. Benedict Prevost and published by M. P. Prevost. A ray of solar light being introduced into a darkened chamber, is to have a square piece of white paper about two inches in the side, passed across it perpendicularly to the direction of the ray. The light reflected by the paper, instead of being white, will present a small white central portion, surrounded by the seven principal colours, nearly in the order of the prismatic spectrum. When a red surface is used instead of a white one, the decomposition of the light is still more complete. When the paper has a slight blue tint, the effect is less perfect than with the white paper. With a black surface no colours appear, but a sort of smoky shade towards the middle. A single passage of the paper is sufficient, but it is necessary that it pass entirely through the ray, no part remaining in it.—*Bib. Univ.*—*Bul. Univ.* A. viii. 248.

11. *On the Barometer.*—The following are conclusions at which M. Bohnenberger has arrived relative to the barometer: i. The surface of mercury in a tube 14.5 lines in diameter, is slightly rounded at the edge; but, at the distance of two lines from the glass, capillary depression disappears, and the surface is level. ii. The mercury in a tube 5.8 lines in diameter, is convex over the whole surface, the depression being .035 of a line. iii. The depression is generally less in a vacuum than in the air, so that a syphon barometer gives results too high, and the more so as the tube is smaller. iv. Barometers constructed with tubes five lines in diameter, do not require tapping to cause them to assume their proper height; and comparatively slight blows easily make the mercury rise too high in tubes of a smaller diameter.—*Annal. der Phys. und Chem.*

12. *Easy Method of reducing Barometrical Observations to a Standard Temperature, by* S. Foggo.—The expansion of mercury deduced by the different philosophers who have examined it, is given below; omitting the results of Sir G. Shuckburgh, as being rather too far from the mean of the others.

Expansion of mercury, from 32° to 212° F.

De Luc..................	1-56th	mean, 1-55.43th.
Lavoisier and Laplace......	1-55.22th	
Halstrom.................	1-55th	
Dulong and Petit..........	1-55.5th	

For 1° of Fahrenheit's scale, this is equal to $\frac{1}{9977}$, or .00010023: which may be called one ten-thousandth, without the most trifling error in practice. The barometric column may, therefore, be reduced to the standard temperature of 32° F. by the following simple rule, which will make a table unnecessary. *Before the first three figures of the observed height place two cyphers, multiply by the temperature of the mercury — 32°, and subtract the product from the observed height.* Example; barometer 30.597, temperature of mercury 74°.

$74° - 32° = 42°$ $.00305 \times 42 = .128$ and $30.597 - .128 = 30.469$ the correct height.

When the temperature of the mercury is lower than 32°, the temperature is to be subtracted from 32°, and the product, obtained as before, is to be *added* to the observed height. Thus, let the barometer be as before, and the temperature 15°: then $32° - 15° = 17$; $.00305 \times 17 = .052$, and $30.597 + 052 = 30.649$, the correct height.—*Jameson's Journal*, 1827, p. 378.

13. *Diamond Lenses.*—I see by the last number of the Journal of Science and the Arts, that Mr. Varley has made a Diamond Lens, and also a single microscope with such motions as enable the observer to follow an animalcule in a diagonal direction. It is very odd, but this is precisely my plan for a microscope, which I drew up about four years ago; and as I could not get any optician to undertake it, I sent it to the Society of Arts, and recommended them to offer a premium for the best diamond lens, but they returned it. I have had a microscope of this sort (made by W. and S. Jones, Holborn) about a year and a half, and it answers the purpose completely; as a person not at all used to microscopes may use a lens of $\frac{1}{60}$ inch focus and find a small object with it, and bring any part of it into the field of view with the greatest facility, and follow the motions of an animalcule in a diagonal direction. There are some alterations and improvements, which I have since made, that have rendered it a very complete microscope; a drawing of which I could send you, if you think it would be acceptable.

I am, Sir, yours, &c.

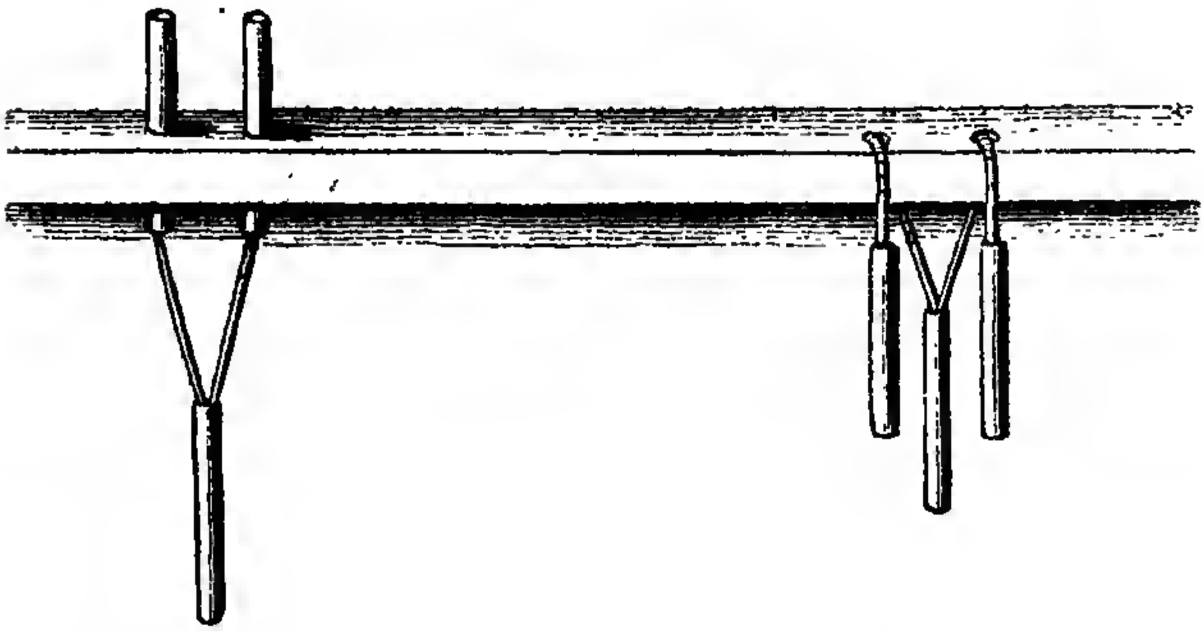
Tringham, Norfolk, July 9th, 1827. G. Dakin.

14. *Sapphire Lenses for Single Microscopes.*—As it may justly be feared that, notwithstanding the incontestable superiority of diamond lenses, the cost and difficulty attendant on their production will enhance their value beyond the reach of the public, Mr. A. Pritchard, No. 18, Pickett Street, has applied himself with indefatigable perseverance to the formation of *Sapphire Lenses.* The valuable experiments of Dr. Brewster have determined that the sapphire possesses a stronger refraction than any other substance capable of giving a single image (diamond excepted),

while its dispersive power is only 0.026 compared to water as 0.035. Thus if a sapphire is ground in the same tool which will form a lens of glass of the $\frac{1}{60}$ inch focus, it will come out about the $\frac{1}{100}$ inch focus; being almost double the power of the glass in linear amplification, and more than double in superficial; in which latter mode of estimation the powers of the glass and sapphire may be rated at 360,000 to 1,000,000. The faint blue tinge of the sapphire is not felt in thin small lenses formed of this substance, which thus come next in order to diamond ones, and form an excellent *pis aller* for those who cannot come at the latter. Many of our first microscopists are already in possession of them, and have honoured them with their unqualified approbation.

There is a property possessed by small single lenses formed by precious stones, which is worthy of being commented on: viz. They can be burnished fast into brass rings, and thus safely cleaned and removed at pleasure from one setting to another. The cohesion of glass is too slight to permit this operation, during which it is almost sure to burst into shivers.—C. R. G.

15. *On a Method of Securing and Preserving the Rowing Pins in Boats.*—Dear Sir,—To remove a petty inconvenience of hourly occurrence, by some simple contrivance, is often productive of a greater mass of advantage than an invention of greater splendour, and of apparently more extensive utility.



In the accompanying drawing, you have a plan for preserving that indispensable requisite in a boat, the towels, or rowing pins; the loss of which is not only very teasing, but often productive of serious inconveniences; while the practice of stealing them from each other forms a constant source of petty depredations, leading to perpetual quarrels among seamen in harbours. He who has been detained the better part of a day in the island of Sky, till half

a dozen of these pins could be procured, well knows how to value that trifle, the neglect of which has caused the loss of his voyage, and might have led to that of his boat and his life also.

Fixed towels cannot well be used when boats are to be hoisted in alongside, as they are subject to be broken; and they are often inconvenient in getting in water casks, as well as in many other cases. Hence, pins capable of being unshipped are preferable. These are frequently lost, and the want is not always discovered till it cannot be replaced; or else it is not replaced without loss of that time which is often so valuable at sea. Very often, also, the delay of even a minute is rendered inconvenient or even dangerous; when the boat is dragging alongside by the painter in a heavy sea, and the vessel is either drifting or standing on.

The drawing requires little explanation. By pulling at the lower pin, the two upper are fixed at once, and on being unshipped they hang secure from loss; while the lower one serves as a spare towel, should any be broken. As not one boat in twenty thousand is provided with this invention, which is indeed scarcely known, it will not perhaps be found undeserving a place in your Journal.—

I am, &c. J. M.

16. *Cold Injection for Anatomical Preparation.*—If a mixture of varnish and vermilion has a small quantity of water mixed with it, it soon sets and becomes hard. This affords an excellent composition for anatomical injection, being very beautiful and very penetrating, (so much so, that it frequently returns by the veins,) and requiring no heat to be applied to the subject. The writer of this article frequently had, in the course of his medical education, the office of preparing this injection, of which he has, however, unfortunately forgot the proportions, and the particular nature of the varnish. It was, he thinks, a spirit varnish; the water was not mixed until the instant the injection was wanted, when it was well worked up with the syringe, and immediately thrown in; in the course of a night it would have set beautifully. This particular kind of injection was invented by an American anatomist of the name of Ramsay, and preserved as a valuable secret by him for the exclusive use of his own dissecting room. The proportions, &c. of the ingredients will soon be attained by a few experiments.

II. Chemical Science.

1. *Extraordinary Experiments on Heat and Steam by* Mr. Perkins.—" I discovered that a generator at a certain temperature, although it had a small crack in it, would not emit either water or steam. This fact I mentioned to a very scientific friend, who questioned its accuracy, and to convince him I tried the experiment; but he concluded that the expansion of the metal must have closed the fissure. To remove every doubt, I proposed to drill a small

hole through the side of the generator, which was accordingly done. After getting the steam up to a proper temperature, I took out the plug, and although we were working the engine at thirty atmospheres, nothing was seen or heard to issue from the plug-hole; all was perfectly quiet: I next lowered the temperature by shutting the damper, and opening the furnace door; a singing from the aperture was soon observable, and when a coal was held before it, rapid combustion ensued; nothing, however, was yet visible: but as the temperature decreased, the steam became more and more visible, the noise at the same increasing, until finally the roar was tremendous, and might have been heard the distance of half a mile. This was conclusive. I should mention that, at the aperture, the iron was red-hot." "The hole was one *quarter of an inch* in diameter."

"The experiment affords some data towards answering the question, at what distance from the heated metal the water remained, when under the pressure of thirty atmospheres; we may safely aver that it exceeded one-eighth of an inch."—*Silliman's Journal,* xiii. 46.

2. *On the Use of feeble Electric Currents, for effecting the Combination of numerous Bodies, by* M. Becquerel.—A highly interesting memoir on this subject is inserted in the thirty-fifth volume of the *Annales de Chimie,* the intention of M. Becquerel being to show that electro-chemical powers may be used not only for the decomposition and analysis of bodies, but also for the production of new compounds.

The facts described in the paper are commenced by one intended to illustrate future reasoning, by shewing what takes place when a very feeble electric current traverses a metallic circuit, interrupted in one part by a neutral solution, into which the two extremities of the wires forming the circuit are immersed. Two small copper wires were connected together by loops, and the two free ends joined to the ends of a galvanometer wire; the circuit was then cut in one place, and the extremities immersed in a solution of chloride of sodium. Then, if one of the loops be raised to a red heat by a spirit lamp, an electric current is produced, the heated loop furnishing negative electricity. Now if the ends plunged in the saline solution are terminated by platina or gold wires, *no current* of electricity is observed; with silver terminations, the current is very feeble; but with wires of zinc, lead, iron or tin, the current is very energetic. These remarkable effects, highly important in the phenomena hereafter to be considered, are no way connected with the conductibility of the metals; for lead and zinc, which are the worst conductors, are those which, with the copper, produce the most powerful effects. The current ceases altogether as soon as the lamp is removed.

As the zinc, copper, lead, and iron, belong to the class of oxidable metals, M. Becquerel concludes, from this experiment, that

when very feeble electricities are generated in any point of a metallic circuit, interrupted by a saline solution, *a current of electricity is formed or not, according as the two similar metallic terminations, which dip into the solution, belong to an oxidable or non-oxidable metal.* If the saline solution be replaced by an acid, *then* a current will be obtained, though platina wires be used; because that kind of fluid does not interrupt the current.

With respect to the production of new compounds by electro-chemical powers, very much depends upon the strength of the power employed, and M. Becquerel only pretends, as yet, to indicate a new field of research, and not to point out the precise paths to be pursued. Two methods may be adopted. As an illustration, let a tube, from 4 to 8 hundredths of an inch in diameter, be bent into the form of the letter U, and place a plug of amianthus at the bend, to prevent the mixture of the fluids in the limbs: into one leg put a mixture of deutoxide of copper and solution of the sulphate of copper, the former will fall to the bottom; into the other put a saturated solution of common sal d also an excess of the dry substance, then communicate the two fluids by a plate of copper. Very shortly the end plunged in the sulphate will be covered with metallic copper, and the acid set free will act upon the oxide of copper below and form more sulphate, so that a set of decompositions and recompositions will occur, and ultimately comparatively large crystals of copper will be obtained.

In the other branch of the tube, a portion of the salt will be decomposed, the muriatic acid will act upon the copper, which is oxidised in consequence of its positive state, and will probably produce an oxychloride, which will combine with the chloride of sodium, and then octoedral crystals will be formed on the plate of copper. The effects are produced either with or without access fair.

When the crystals are well dried and inclosed in a tube hermetically sealed, they suffer no change; but they are decomposed by water into chloride of sodium and submuriate of copper.

If the voltaic experiment be continued for one or two months, the crystals, from being colourless and limpid, become violet, and ultimately acquire an emerald green hue, still remaining transparent. If the chloride of sodium side be tested, it will be found that soda is evolved during the experiment. A piece of copper simply immersed in a solution of common salt, produces nothing more than a submuriate of copper, which precipitates.

With silver.—If a similar tube to that described have both limbs filled with a solution of salt, a platina wire introduced into one limb, a silver wire into the other, the extremities of the wires connected so as to form a voltaic circuit, and the whole [illegible] for some months, in about fifteen days crystals will [illegible] on the silver wire; these will gradually increase and [illegible] boidal forms. They have not yet been part[illegible]

are known to be unchanged by water: during a long experiment they change colour, becoming, first, violet, then blue.

Experiments similar to that with the copper, when repeated with the same solutions, &c., but the substitution of plates of lead and tin for the copper plates, produced crystalline double chlorides of these metals and sodium.

Muriate of ammonia being substituted for common salt in these experiments, another series of double compounds was obtained with copper, silver, lead, and zinc.

A double chloride of barium and lead was formed slowly in a similar way.

When a solution of the iodide of potassium or sodium was used instead of the solution of salt, then double iodides were obtained: thus with lead rather a rapid formation of silky crystals occurred upon the lead, which, when examined by water, were decomposed, producing iodide of lead and solution of iodide of potash or soda. A tube two or three times the diameter of the former may be used for the experiment.

The second method of producing new combinations by weak electro-chemical powers, depends upon the electro-motive action, which is caused whenever a metal touches the oxides, or an oxide of another metal. If an oxide of a metal, a plate of metal, and a liquid be put into a tube closed at one extremity, there will be an electro-motive action of the metal with the oxide, and of the liquid with both these bodies; and the chemical effect will be according to the resultant of these three forces, which can only be ascertained by experiments.

As an illustration of the effects thus produced, three tubes, from eight to twelve hundredths of an inch in diameter, were prepared, a little protoxide of lead being put into one, deutoxide into the second, and peroxide into the third; solution of muriate of ammonia and a plate of lead were then added to each tube. After a time, lead was precipitated in the first tube, very slight chemical changes took place in the second, but a large quantity of double chloride of lead and ammonia crystallized upon the lead in the third, in the form of needles. Thus very different effects were produced, according to the state of oxidation.

Solution of salt gave similar results with the oxides of lead and lead.

The oxides of copper, with solutions of alkaline muriates, gave curious results. With muriate of ammonia, crystals were produced of considerable size, and different to those obtained by the former process. In this experiment, the black and anhydrous deutoxide of copper gradually acquired a blue colour, as if a hydrate were formed under the influence of the feeble electric current formed by the arrangement.

Copper, its deutoxide, and solution of corrosive sublimate, produced a double chloride, crystallizing in plates, and possessing a metallic lustre.

3. *Crystallization of Metallic Oxides.*—If a solution of nitrate of copper, mingled with very fine charcoal powder, or even deutoxide of copper, be put into a similar tube to that described in the last article, then a plate of copper be introduced and the vessel closed up, in about fifteen days small red transparent octoedral crystals of protoxide of copper will be formed on the plate of metal. Other metals have been subjected to similar experiments, but probably have not yet remained long enough under action.—*Ann. de Chimie*, xxxv. 113.

4. *On Bromine, by* M. A. de la Rive.—M. de la Rive has remarked a curious fact respecting the conducting power of fluids for electricity in the habitudes of bromine and water. He found, in the first place, as M. Balard had stated, that pure dry bromine did not conduct the electricity of a voltaic battery, consisting of sixty pairs of plates very strongly charged, a delicate galvanometer being the test: a similar experiment was then made with pure water, the water being contained in a glass capsule, and communicated with the battery and galvanometer by platina wires*, and the deviation of the needle was scarcely sensible. Some other experiments induced M. de la Rive to believe, that water perfectly distilled and put into vessels made of substances absolutely unacted upon, would not conduct any portion of electricity: the purer the water, and the more unchangeable the substance of the vessel, the feebler does the conducting power become, until at last it is insensible.

A few drops of bromine were then added to the water, which soon acquired a yellow colour, by dissolving a small portion of the substance; being now included in the voltaic circuit, the galvanometer needle was deviated 70°, and an abundant disengagement of gas took place from the platina wires. These were oxygen and hydrogen, in the usual proportion, proving that the water only had been decomposed.

From these experiments it results, that a body which does not at all conduct voltaic electricity, or, at least but very badly, namely, *pure water*, may be rendered a very good conductor, by its mixture with a few drops of perfectly *non-conducting* substance, namely, *bromine*. M. de la Rive has found the same fact to occur with iodine, and iodine and water; and his father had observed, in a course of experiments made a long time ago on the conducting power of fluids, that diluted sulphuric acid is a better conductor than very much concentrated acid: may not anhydrous sulphuric acid then be a non-conductor like bromine, &c.?—*Annales de Chimie*, xxxv. 161.

5. *Elementary Nature of Bromine.*—Iodine colours a solution of starch blue, bromine renders a similar solution orange colour. M. A. de la Rive added a few drops of bromine to a solution of starch

* See, on this point, the statement by M. Becquerel, p. 462, relative to the use of platina wires, when forming a communicating medium with fluids.

coloured blue by iodine, and obtained a compound which gave two distinct colours with starch, one brown, the other yellow; the difference of colour corresponding with the two bromides of iodine described by M. Balard. These compounds of iodine and bromine, dissolved in a solution of starch, were subjected to the voltaic pile: immediately the yellow solution became blue about the negative pile, and orange about the positive pile, indicating the separation and places of the iodine and bromine. Thus the smallest quantity of iodine may be discovered in bromine; but when the experiment was resorted to, to prove whether the idea thrown out, that bromine was a compound of chlorine and iodine, was founded in fact or not, it gave no such indication, and a solution of bromine in starch electrified for a long time together, gave no appearance of iodine. Hence M. de la Rive concludes, that bromine contains no iodine, but is an element analogous to iodine and chlorine.

When bromine and iodine are combined, the former passes to the positive pole, and is consequently more negative than the latter; which accords with the observation of M. Balard, that it should occupy a place between chlorine and iodine.

According to the *Bulletin Universelle*, when the letter to M. Arago, containing an account of the facts above referred to, was read to the Academy of Sciences, that body decided that the assertion of M. Dumas that bromine was a compound of chlorine and iodine should be considered as retracted, and that it should be so entered, upon the procès-verbal of the sitting.—A. viii. 209.

6. *Quantity of Bromine in Sea-Water.*—One hundred pounds of sea-water, taken up at Trieste, treated by chlorine, ether, &c., according to M. Balard's process, produced five grains of bromide of sodium, or 3.278 grains of bromine. It would appear that, in the sea-water of Trieste, the bromine is unaccompanied by any iodine, and the same is the case, according to M. Hermbstadt, with the waters of the Dead Sea. In the water of the Mediterranean, on the contrary, iodine always appears with the bromine.

7. *Sale of Bromine.*—The discoverer of bromine, M. Balard, has been enabled, by his improvements, to prepare that peculiar body in quantities sufficient to permit its sale. It may be obtained at his shop, Rue Argenterie à Montpellier, or at M. Quesneville's manufactory of chemical substances at Paris. The price is four francs the gros (about 60 grains), fourteen francs the half ounce, and twenty-three francs the ounce.

8. *Preparation of Iodous Acid.*—M. Pleischl says that, in preparing this acid, three parts of chlorate of potash with one of iodine are to be used, and not equal parts according to M. Sementini; and also that it is indispensable to cool the receiver considerably during the whole operation.

9. *On a peculiar Nitric Acid, and Sulphate of Potash, by* Mr. Phillips.—For the purpose of preparing nitric acid of the greatest strength, Mr. Phillips mixed 70 parts of nitre with 70 parts of oil of vitriol, S. G. 1.8442 at 60°. and distilled for eight hours. The nitric acid obtained was reddish yellow, weighed 46.13 parts, was of S. G. 1.5033, and by an experiment on carbonate of lime, was found equivalent to 34.24 of that substance; the latter fact indicates that 36.98 of real acid was present, and the liquid acid therefore consisted of

Real nitric acid............	36.98	or	80.16
Water	9.15	..	19.84
	46.13		100.00

Supposing this acid to be a definite compound of two atoms of acid, 108, and three of water 27, it would consist of

Real acid	36.90	or	80
Water	9.23	..	20
	46.13		100

The salt remaining in the retort weighed 92.87 parts; nearly this weight of water being added and heated, the whole was dissolved, and on cooling, a salt, consisting of extremely minute filaments resembling asbestos, was obtained, which, by capillary attraction, retained a part of the residual solution so powerfully, that it was necessary to absorb it by filtering paper.

Although it appeared improbable that the crystals could be a variety of the known form of bisulphate of potash, yet supposing it might be that salt with either less, or more than two atoms of water, Mr. Phillips proceeded to its analysis. Some of the salt was readily dried by exposure to the air of a warm room: 100 grains, by muriate of baryta gave 154.75 grains of sulphate of baryta, equivalent to 52.45 sulphuric acid: 100 grains heated to redness, lost 21.6 sulphuric acid and water, and left 78.4 grains of neutral sulphate of potash. The latter contain 35.6 grains of sulphuric acid, which, subtracted from the whole quantity of 52.45, indicates 16.85 as the quantity dissipated by heat; and this again, subtracted from the 21.6, indicates 4.75 water in the crystals. The quantity of acid separated by heat is, therefore, very nearly half that remaining in the neutral sulphate, and the salt in question appears to be a sesquisulphate of potash, consisting of

			theory.		experiment.
3 atoms sulphuric acid	120	....	55.33	....	52.45
2 „ potash	96	....	42.66	....	42.80
1 atom water	9	....	4.00	....	4.75
	225		99.99		100.00

Mr. Phillips found it difficult to prepare the sesquisulphate fr[illegible]

from bisulphate; and on repeating the attempt to procure it exactly as before, obtained a large quantity of bisulphate, and a small quantity of the peculiar salt; although the quantity of water present is known to have an important influence on the nature of the sulphates produced, yet the precise circumstances on which the formation of sesquisulphate depends, are at present unknown.—*Phil. Mag. N. S.*, ii. 429.

10. *On certain Properties of Sulphur.*—The effect of heat upon sulphur in first fusing it, but afterwards causing diminution of fluidity in a certain degree proportionate to the temperature, has been long and generally known, as well also as the peculiar soft state into which the sulphur may be brought, by pouring it, when hot and thickened, into cold water. M. Dumas has been led to examine these phenomena for the purpose of acquiring a precise and particular knowledge of the effects and changes.

Fused sulphur began to crystallize between 226° and 228°. Its fusing point may be considered as 226°.4. Between 230° and 284° it is as liquid as a clear varnish, and of the colour of amber; at about 320° it begins to thicken, and acquire a red colour; on increasing the heat, it becomes so thick, that it will not pour. This effect is most marked between 428° and 572°; the colour being then a red-brown. From 572° to the boiling point it becomes thinner, but never so fluid as at 248°. The deep red-brown colour continues until it boils.

When the most fluid sulphur is suddenly cooled, it becomes brittle, but the thickened sulphur, similarly treated, remains soft, and more soft as the temperature has been higher. Thus, at 230°, the sulphur was very liquid, and yellow; and cooled suddenly by immersion in water, it became yellow and very friable; at 374° it was thick, and of an orange colour, but by cooling, became at first soft and transparent, but soon friable, and of the ordinary appearance; at 428°, it was red and viscid, and when cooled, soft, transparent, and of an amber colour; at the boiling point it was deep brown red colour, and when cooled very soft, transparent, and of a red-brown colour.

It is not necessary, as is sometimes stated, to heat the sulphur a long time to produce this effect; all depends upon temperature. The only precaution necessary is, to have abundance of water, and to divide the sulphur into small drops or portions, that the cooling may be rapid. If it be poured in a mass, the interior cools slowly, and acquires the ordinary hard state. When the experiment is well made at 446°, the sulphur may be drawn into threads as fine as a hair, and many feet in length.

M. Dumas, in remarking upon this curious effect of sudden cooling, classes it with the similar effect which occurs with bronze. Although difficult to assign the exact cause, yet he notices that the tendency to crystallize can evidently be traced as influential over some of the appearances, the hardness and opacity, for instance,

which always occur together when the crystalline state is assumed; whereas, when rapid cooling has hindered crystallization, the mass remains soft and transparent, until it crystallizes, which usually happens in twenty or thirty hours.—*Ann. de Chimie*, xxxvi. 83.

11. *On the Fluidity of Sulphur and Phosphorus at common temperatures*, by Mr. Faraday.—I published some time ago a short account of an instance of the existence of fluid sulphur at common temperatures*; and though I thought the fact curious, I did not esteem it of such importance as to put more than my initials to the account. I have just learned, through the *Bulletin Universel* for September, p. 178 †, that Signor Bellani had observed the same fact in 1813, and published it in the *Giornale di Fisica*, vol. vi. (old series). I also learn, by the same means, that M. Bellani complains of the manner in which facts and theories, which have been published by him, are afterwards given by others as new discoveries; and though I find myself classed with Gay-Lussac, Sir H. Davy, Daniell, Bostock, &c., in having thus erred, I shall not rest satisfied, without making restitution, for M. Bellani, in this instance, certainly deserves it at my hand.

Not being able to obtain access to the original journal, I shall quote M. Bellani's very curious experiments from the Bulletin, in which they appear to be fully described. " The property which water possesses, of retaining its fluid states, when in tranquillity, at temperatures 10° or 15° below its freezing point, is well known; phosphorus behaves in the same manner; sometimes its fluidity may be retained at 13° (centigrade?) for a minute, an hour, or even many days. What is singular is, that, though water cooled below its freezing point, congeals easily upon slight internal movement, however communicated, phosphorus, on the contrary, sometimes retains its liquid state even at 3°, even though it be shaken in a tube or poured upon cold water. But, as soon as it has acquired the lowest temperature which it can bear without solidifying, the moment it is touched with a body at the same temperature, it solidifies so quickly, that the touching body cannot penetrate its mass. If the smallest morsel of phosphorus is put into contact with a liquified portion, the latter infallibly solidifies, though it be only a single degree below the limit of temperature necessary; this does not always happen when the body touching it is heterogeneous.

" Sulphur presented the same phenomena as phosphorus; fragments of sulphur always produced the crystallization of cold fluid portions. Having withdrawn the bulb of a thermometer which had been plunged into sulphur at 120°, it came out covered with small globules of sulphur, which remained fluid at 60°; and having touched these one after another with a thread of glass, they became solid: although several seemed in contact, yet it required that each

* Quarterly Journal of Science, xxi. 392.

† The Italian Journal has not yet arrived in this country.

should be touched separately. A drop of sulphur, which was made to move on the bulb of the thermometer, by turning the instrument in a horizontal position, did not congeal until nearly at 30°; and some drops were retained fluid at 15°, *i. e.* 75° of Reaumur below the ordinary point of liquefaction."

The *Bulletin Universel* then proceeds to describe some late and new experiments of M. Bellani, on the expansion in volume of a cold dense solution of sulphate of soda during the solidification of part of the salt in it. The general fact has, however, been long and well known in this country and in France; and the particular form of experiment described is with us a common lecture illustration. The expansion, as ascertained by M. Bellani, is $\frac{2}{87}$ of the original volume of fluid.

According to the Bulletin, M. Bellani also claims, though certainly in a much less decided manner than the above, the principal ideas in a paper which I have published on the existence of a limit to vaporization, and I referred back to the *Giornale di Fisica* for 1822, (published prior to my paper,) for the purpose of rendering justice in this case also. Here, however, the contact of our ideas is so slight, and for so brief a time, that I shall leave the papers in the hands of the public without further remarks. It is rather curious to observe how our thoughts had been at the same time upon the same subject. Being charged in the Bulletin with quoting an experiment from a particular page in M. Bellani's memoir, (which I did from another journal, in which the experiment only was described,) I turned to the original place, and there, though I found the experiment I had transferred, I also found another which I had previously made on the same subject, and which M. Bellani had quoted.

I very fully join in the regret which the *Bulletin Universel* expresses, that scientific men do not know more perfectly what has been done, or what their companions are doing; but I am afraid the misfortune is inevitable. It is certainly impossible for any person who wishes to devote a portion of his time to chemical experiment, to read all the books and papers that are published in connexion with his pursuit; their number is immense, and the labour of winnowing out the few experimental and theoretical truths which in many of them are embarrassed by a very large proportion of uninteresting matter, of imagination, and of error, is such, that most persons who try the experiment are quickly induced to make a selection in their reading, and thus inadvertently, at times, pass by what is really good.

12. *Separation of Selenium from Sulphur.*—Berzelius says, that these substances, so much resembling each other in their general properties, may be easily separated by the following process. When sulphuret of selenium is fused with carbonate of potash, the alkali not being in excess, the fused mass, dissolved in water, leaves selenium undissolved and free from sulphur.

Some of the sulphuret of selenium from Lukawitz, in Bohemia, was dissolved in potash, and the solution converted into hyposulphite by exposure to the air at the temperature of 65° F.; 0.1125 of the sulphuret experimented with were precipitated, and found to be *pure selenium*. The solution being of a deeper red colour than that of the common sulphuret, a piece of sulphur was put into it, and the whole boiled for a moment; a quarter of a grain of selenium, perfectly free from sulphur, was precipitated.

A solution of a neutral seleniate, or of one with excess of base, is soon rendered turbid by having sulphuretted hydrogen passed through it. At first pure selenium separates; afterwards sulphuret of selenium; and, lastly, mere sulphur. The solution should be considerably diluted; when concentrated, the precipitate formed is of a flame yellow colour, but soon becomes brownish-black, and sulphur is deposited, sometimes crystallizing at the surface of the deposite.—*Phil. Mag., N. S.*, ii. 390.

13. *On a new Compound of Selenium and Oxygen—Selenic Acid, by* MM. Mitscherlich and Nitzsch.—This acid contains half as much more oxygen as that discovered by M. Berzelius, and with potash forms a neutral salt, having the same form and optical properties as sulphate of potash, containing no water when crystallized, and producing insoluble precipitates with barytic salts. The acid is isomorphous with the sulphuric, and may with propriety be called *selenic acid*, that described by M. Berzelius being considered as the *selenious acid*.

The new acid is easily prepared: for this purpose selenium, selenious acid, a selenite or a metallic selenuret is to be fused with nitre. Selenuret of lead, being the most abundant source, has been used for this purpose, but being accompanied by sulphuret, the selenic acid is usually contaminated by sulphuric acid. The selenuret of lead is to be freed from carbonates by muriatic acid, and the residue mixed with its weight of nitrate of soda, and thrown gradually into a red-hot crucible. Water then dissolves out seleniate nitrate and nitrite of soda, no selenium remaining in the residue The solution quickly boiled, deposits anhydrous seleniate of soda, and this being separated, by cooling crystals of nitrate of soda are formed; these being removed, ebullition again causes more seleniate to fall down, and proceeding in this way an imperfect separation is effected. The seleniate, like the sulphate of soda, is most soluble in water at 181°. To purify the salt completely, the nitrite should be changed into nitrate by nitric acid; but then sulphate of soda would remain as an impurity formed from sulphuret in the ore, and no attempt to separate this has as yet succeeded.

But if the seleniate of soda be mixed with muriate of ammonia and heated, selenium, nitrogen and water come over, no trace of sulphur appearing. The selenium may, however, be dissolved in excess of nitric acid, and the selenious acid produced tested by

muriate of baryta, which would then separate sulphuric acid if present; the clear solution is to be saturated with carbonate of soda, evaporated to dryness, and the mixture of selenite and nitrate of soda obtained, fuzed in a porcelain crucible over a spirit-lamp. Then proceed by crystallization as before, and a pure seleniate of soda will be produced.

To separate the selenic acid, the solution is to be decomposed by nitrate of lead; the seleniate of lead is as insoluble as the sulphate, and being well washed, is to be decomposed by a current of sulphuretted hydrogen, which has no action on the selenic acid; the solution being filtered, is to be boiled, and is then diluted selenic acid. Its purity, as respects fixed bodies, is ascertained by its entire volatility; if sulphuric acid be present, it may be ascertained by boiling a portion with muriatic acid, which produces selenious acid, and then testing by muriate of baryta, a precipitate indicates sulphuric acid.

From the isomorphism of selenic acid and its salts with sulphuric acid and its salts, M. Mitscherlich concluded, that the oxygen in the acid should be to that in selenious acid as 3 to 2; and to that in bases when it forms salt, as 3 to 1. These views were confirmed by experiments. From the decomposition of seleniate of potash by muriate of baryta, it appeared that the seleniate was composed of

Potash	42.16	oxygen	7.15
Selenic acid ..	57.84	——	21.79
	100.00		

The composition of the acid was determined by boiling a certain weight of the seleniate of soda with muriatic acid in excess, and decomposing the selenious acid formed by sulphite of soda; 4.88 of the salt gave 2.02 of selenium, from which, and the above result, it would appear that the acid is formed of

Selenium	61.4
Oxygen	38.6
	100.0

According to Berzelius, selenious acid consists of 100 selenium, and 40.33 oxygen; and supposing this contains two-thirds the oxygen in selenic acid, the latter should consist of 62.32 and 37.68. From the analysis above given of the seleniate of potash, it is evident that 100 of selenic acid saturates a quantity of base containing 12.56 of oxygen, which would agree with the latter estimate of selenic acid.

Selenic acid is a colourless liquid, which may be heated to 536°, without sensible decomposition; above that it changes, and is rapidly resolved into oxygen and selenious acid at 554°. Heated to 329°, its specific gravity is 2.524; at 512°.6 it is 2.6; at 509° it is 2.625; but by that time selenious acid has been formed in it. A portion of concentrated acid, from which the selenious acid had

been removed, consisted of 84.21 selenic acid, and 15.75 water; but it is certain that the selenic acid begins to decompose before it has resigned the last portions of water.

Selenic acid has a powerful attraction for water, and evolves much heat when mixed with it. It is not decomposed by sulphuretted hydrogen; so that the latter body may be used to decompose the seleniates of lead and copper. When boiled with muriatic acid it produces selenious acid and chlorine, and the mixture, like aqua regia, will dissolve gold or platina. Selenic acid dissolves zinc and iron, evolving hydrogen; it dissolves copper, evolving selenious acid; and it dissolves gold, but not platina. Sulphurous acid has no action on selenic acid, but instantly decomposes the selenious acid. A solution containing selenic acid is easily decomposed, by first boiling it with muriatic acid, and then adding sulphurous acid.

Selenic acid is but little inferior to sulphuric acid in its affinity for bases; seleniate of baryta is not completely decomposed by sulphuric acid. Its combinations being isomorphous with those of sulphuric acid, and possessing the same crystalline forms, and the same general chemical properties, present but very slight, though very interesting differences from the sulphates. These will be resumed by M. Mitscherlich in a future memoir, with the express object of illustrating the theory of Isomorphism.—*Ann. de Chimie*, xxxvi. 100.

14. *Preparation of Hyposulphuric Acid.*—According to M. Heeren, to obtain the greatest quantity of this acid in the process of passing sulphurous acid over black oxide of manganese, the temperature should be low, and the oxide finely divided. The largest portion of hyposulphuric acid is formed at the commencement of the operation.

15. *Singular Habitude of Phosphoric Acid with Albumen.*—MM. Berzelius and Englehart differed in their results respecting the effect of phosphoric acid on albumen; the latter found the acid caused precipitation of the substance, the former the reverse. Fortunately coming into company, they made some experiments, and discovered a very singular property of the acid. The acid in Berzelius's laboratory not precipitating albumen, Dr. Englehart prepared a fresh portion from phosphorus and nitric acid, evaporating the solution in a platina vessel, and heating it to redness. This acid, dissolved in water, precipitated both animal and vegetable albumen abundantly. Another portion of acid, prepared by burning phosphorus in air, also precipitated albumen. After many experiments to discover the cause of difference in the acids, Dr. Englehart remarked, that the two acids he had prepared, gradually lost their power of precipitating albumen, and in some days were like the acid of Berzelius. This change took place both in open and closed vessels, and was not at all hastened by ebullition,

Upon evaporating the acid, and heating it to redness, it recovered its precipitating power, but gradually lost it again by a day's repose. The cause of this difference escaped detection; it evidently does not depend upon a difference of oxidation. "May it not be supposed," says Berzelius, "that there exists a chemical combination of phosphoric acid with water, which is not formed until some time after solution, and which is incapable of precipitating albumen?"—*Annales de Chimie*, xxxvi. 110.

16. *Economical Preparation of Deutoxide of Barium.*—This process is due to M. Quesneville. Nitrate of baryta is to be put into a luted earthenware retort, to which a tube is to be attached for the purpose of conveying the liberated gases to a water-trough. The retort is to be gradually heated to redness, and retained at that temperature as long as nitrous acid and azotic gas pass over; the evolution of these substances indicates that nitrate of baryta still remains to be decomposed, but the instant that pure oxygen gas passes off, the fire is to be removed and the retort cooled. The product of this decomposition is a peroxide of barium; it falls to pieces in water, without producing heat, disengages oxygen when boiled with water, and is reduced to a protoxide by a strong heat. When acted upon by sulphuric acid, no nitric acid was evolved; and when subjected to nitric acid, no nitric oxide was produced. The production of this peroxide is easily understood, for the protoxide formed by the decomposition of the nitrate being in contact, at a red heat, with a large quantity of oxygen in a nascent state, combines with it, and is retained, unless the heat be so high as to decompose it.—*Annales de Chimie*, xxxvi. 108.

The decomposition and effect are precisely the same as those lately pointed out by Mr. Phillips as occurring with potassium when the nitrate of potash is decomposed by heat.—See p. 483 of the last volume of this Journal.

17. *Preparation of Aluminum—Chloride of Aluminum.*—According to the accounts published, the following process has succeeded in the hands of M. Oersted, in decomposing alumina and evolving the base *aluminum*. Pure alumina is to be heated to redness, and then well mixed with pulverized charcoal; the mixture is to be placed in a porcelain tube, and being heated to redness, is to have dry chlorine gas passed over it; the charcoal reduces the alumina, the base combines with the chlorine, and oxide of carbon is formed. The chloride of aluminum is soft, crystalline, and evaporates at a temperature a little above 212° Fahrenheit: it readily attracts moisture from the atmosphere, and becomes hot when water is added to it. Being mixed with an amalgam of potassium, containing much of the latter metal, and immediately heated, chloride of potassium is formed, and the metallic base of the alumina combines with the mercury. The amalgam quickly oxidises by exposure to air; but, being heated out of contact with the atmosphere, the mercury is

volatilized, and a metallic button is left, having the colour and splendour of tin. A fuller account of the researches of M. Oersted on this subject is expected.—Heusmann's *Repertoire*—*Phil. Mag. N. S.* ii.

18. *Mutual Action of Lime and Litharge.*—M. Fournet heated a mixture consisting of 7.12 parts of calcined lime, and 27.89 parts of litharge, very strongly; a coherent mass was obtained, which, pulverized and digested in water, gave, when filtered, a perfectly clear and colourless liquor, which, when treated with sulphuretted hydrogen, threw down an abundant black precipitate: hence oxide of lead is rendered soluble in water by means of lime.—*Ann. des Mines*, i. 538.

19. *New Chloride of Manganese discovered by* M. J. Dumas.—This chloride corresponds in proportions to the manganesic acid, and in contact with water, produces muriatic and manganesic acids. It is easily obtained by putting a solution of manganesic acid into contact with concentrated sulphuric acid, and fused common salt. Water and the new chloride are formed; the former is retained by the acid, the latter volatilizes in a gaseous form. The body does not, however, appear to constitute a permanent gas*, for though, when produced, it appears as an elastic fluid having a cupreous or greenish tint, yet when passed into a tube, cooled to 5° or 4° Fahrenheit, it condenses into a liquid of a brownish green colour.

When the perchloride is produced in a large tube, its vapour gradually displaces the air present, and the tube becomes filled with it; if it then be poured into a jar with moistened sides, the colour of the gas changes as it comes into contact with the moist air; a thick smoke of a fine rose colour appears; and the sides of the vessel acquire a deep purple colour due to the manganesic acid formed. The water thus coloured is abundantly precipitated by nitrate of silver, and, acted upon by a solution of potash, produces all the changes of the mineral chamelion.

The most simple process for the preparation of this body appears to be to form a common green chamelion, to convert it into red chamelion by sulphuric acid, and to evaporate the solution, which will give a residue consisting of sulphate and manganesate of potash. This mixture, acted upon by concentrated sulphuric acid, produces the solution of manganesic acid, into which the common salt is to be thrown in small pieces, until the vapours which rise are colourless; the latter effect is a sign that all the manganesic acid is decomposed, and that muriatic acid only is produced.

An analogous compound is formed when a fluoride is used in place of the common salt. But all attempts as yet made to collect a sufficient quantity for examination have failed; the chloride, on the contrary, is easily formed and examined, although it is not so easy to preserve it.—*Annales de Chimie*, xxxvi. 81.

* Query, what is a permanent gas?—Edit.

20. *Preparation of pure Oxide of Zinc, by* M. Hermann.—It is by no means easy to obtain this substance perfectly pure; the following is M. Hermann's process: Oxide of zinc, or metallic zinc, is to be dissolved in excess of sulphuric acid, and the solution being filtered, sulphuretted hydrogen is to be passed through, so long as a brown or yellow precipitate is formed. Cadmium, lead, or copper, being thus separated, and the solution filtered, it is to be treated with solution of the chloride of lime, (bleaching powder,) by which the iron and manganese will be separated. The solution, again filtered, is then to be crystallized in porcelain vessels, by which sulphate of lime is rejected, and a mother liquor separated, which usually contains cobalt and nickel. The crystals of sulphate of zinc are to be dissolved in as small a quantity of cold water as possible, and the sulphate of lime filtered out; then the solution, being rendered more dilute, is to be decomposed by carbonate of soda in slight excess, and the precipitate well washed, dried, and heated to redness: it is then a perfectly pure and beautifully white oxide.—*Bull. Univ.* A. viii. 263.

21. *Deuto-Sulphuret of Cobalt.*—Mix finely divided oxide of cobalt with three times its weight of sulphur, and heat to very dull redness, until no more sulphur sublimes. The deuto-sulphuret consists of 100 cobalt + 109 sulphur; it is black; is reduced to gray proto-sulphuret by a strong heat.—*Sitterberg.*

22. *Separation of Bismuth from Mercury by Potassium.*—M. Serullas has pointed a striking instance of the separation of bismuth from mercury. He says a twelve hundred thousandth, and even less of bismuth, when dissolved in mercury, may be separated and rendered visible by the addition of a certain quantity of the amalgam of potassium and a little water. A black powder is observed to rise from the substance of the metal, and is a mixture of bismuth and mercury in a very divided state; it rises to the surface or adheres to the vessels.

Copper, lead, tin, and silver, are equally separated, but not so promptly, or so evidently to the eye as bismuth; for they are not associated with divided mercury, at the time of their separation, like the latter: with bismuth a mere atom is rendered visible, and M. Serullas thinks that chemistry does not present a more delicate test than the amalgam of potassium for bismuth in mercury.—*Annales de Chimie*, xxxiv. 195.

23. *Sulphuret of Arsenic proportionate in Composition to Arsenic Acid.*—M. Pfaff acted upon arsenious acid by nitro-muriatic acid, and obtained a pure arsenic acid soluble in water, and deliquescent in the air. This, dissolved in 40 parts of water, and a current of sulphuretted hydrogen passed through it, which instantly produced a yellow orange precipitate of a pulverulent form, continuing identical in composition, until no further precipitate was

occasioned. The fluid was then perfectly free from arsenic. The precipitate was pure sulphuret of arsenic, soluble in ammonia when slightly heated, and composed of equal parts of sulphur and the metal.

M. Pfaff further says that arsenic acid may be separated from its combinations with bases, by dissolving the arseniates in nitric acid, and passing sulphuretted hydrogen through the solution; an abundant precipitate of sulphuret of arsenic is formed, containing no trace of the base of the arseniate decomposed.—*Bull. Univ.* A. viii. 256.

24. *New Double Chromates.*—Mr. Stokes has obtained several new salts, by mixing chromate of potash with metallic sulphates. Chromate of potash, mixed with sulphate of zinc, gave a precipitate of chromate of zinc; and the mother liquor, by concentration, yielded certsin yellow crystals in the form of a flat rhombic prism, which Dr. Thomson had mistaken for impure sulphate of zinc, but which Mr. Stokes recognised as a new compound: 50 grains gave 18.33 sulphuric acid; 0.18 chromic acid; 9.87 oxide of zinc; 8.91 potash; 12.6 water: 0.11 loss.

Chromate of potash and sulphate of nickel were mixed in atomic proportions, and the solutions heated; after the chromate of nickel was separated, they were evaporated to dryness. The residuum, digested in water, was filtered, and the deep red solution obtained upon cooling, yielded grass green crystals in the form of oblique rhombic prisms; 50 grains of these, when analysed, gave 12.26 sulphuric acid; 0.978 chromic acid; 8.2 oxide of nickel; 9.862 potash; 12.7 water.

A similar salt may be obtained by mixing chromate of potash and sulphate of copper. It is of a light green colour, and has precisely the same form as the salts already described. In every case crystals of bichromate of potash were produced in the second crop crystals.—*Phil. Mag. N. S.* ii. 427.

25. *Dobereiner's finely divided Platina.*—The following is M. Dobereiner's process for obtaining finely divided platina, fit for the performance of the experiment which he first made on the combination of oxygen and hydrogen, at common temperatures. Mix muriate of platina with a solution of neutral tartrate of soda in a glass tube, half or three-quarters of an inch in diameter, and twenty or thirty inches in length, and apply heat until the fluid becomes slightly turbid; afterwards expose it for several days to the sun's rays. The greater part of the platina will separate from the solution, and be deposited in minute laminæ, of a greyish black colour on the sides of the glass; the tube and its contents are to be put into a glass vessel containing water, and it is to be filled with hydrogen gas; the platina becomes almost immediately white and shining like silver, and may then be readily detached from the glass. During the reduction of the platina the tartaric acid is partly converted into carbonic and formic acids." As the inflamma-

tion of the hydrogen," it is said, "is caused by abstracting a portion of the caloric from the oxygen, effected by the platina, the smaller the laminæ of the metal are, the more readily is the incandescence produced." Spongy platina for the lamps for instantaneous light, is prepared of great power, by moistening the muriate of ammonia and platina with a concentrated solution of ammonia; the paste formed is to be heated to redness in an earthen or platina crucible.—*Hensman's Repertoire—Phil. Mag. N. S.* ii. 388.

26. *New Metals.*—Professor Osann, of Dorpat, is said to have discovered three new metals in the crude platina, obtained from the Uralian mountains. One, which has occurred only in one specimen of the ore, resembles osmium in some of its compounds. The second forms white acicular crystals from a nitro-muriatic acid solution; these, when heated, being softened and reduced. The third is insoluble in nitro-muriatic acid, and, by a particular process yields a dark green-coloured oxide. The account as yet given of these substances is not precise enough to allow of any judgment respecting their claim to the character of new metals.

27. *Analysis of Porcelain, Pottery, &c. by* M. Berthier.—Earthenware manufactures are divided by M. Berthier into three kinds, those of 1. Porcelain; of 2. Pottery; and of 3. Crucibles, Bricks, &c. The following is the composition of certain porcelains:

PORCELAIN.

	Sèvres. (i.)	English. (ii.)	Piedmont. (iii.)	Tournay. (iv.)
Silica	0.596	0.770	0.600	0.753
Alumina	0.350	0.086	0.090	0.082
Potash	0.018	..	..	0.059 (Potash and Soda)
Soda				
Lime	0.024	0.012	0.016	0.100
Magnesia		0.070	0.152	..
Water	0.008	0.056	0.136	0.006
	0.996	0.994	0.994	1.000

(i.) Sèvres service—Paste strongly heated. It is formed from 0.63 washed kaolin of Limoges; 0.105 quartz sand; 0.052 Bougeval chalk; 0.21 of the fine sand obtained from kaolin by washing, and which is a mixture of quartz and felspar. The glaze of this ware is made of a rock composed of quartz and feldspar. When reduced to a fine powder, it is found to be composed of silica .780, alumine 162, potash 84, water 6: it fuses into a perfectly transparent and colourless glass.

(ii.) Worcester porcelain—Paste taken from the workshops, unbaked.

(iii.) Porcelain of Piedmont—Paste dried. The base of this manufacture is the *magnesite* of Baldissero.

(iv.) Porcelain of Tournay—Clay, chalk, and soda enter into its composition. It is very fusible, but not very fragile.

Pottery.

	Nevers. (i.)	Paris. (ii.)	Gergovia. (iii.)
Silica	0.572	0.511	0.544
Alumina	0.124	0.127	0.220
Lime	0.226	0.063	0.064
Oxide of Iron	0.066	0.070	0.098
Magnesia	..	0.024	0.038
Water	..	0.173	0.020
	0.988	0.998	0.984

(i.) Earthenware of Nevers—Paste of a pale red. Made of a marle occurring close to the town; the glaze is a white enamel, containing both tin and lead.

(ii.) Paste of the brown earthenware made by M. Husson at Paris. The biscuit is red, but is covered by a brown glaze, coloured by oxide of manganese.

(iii.) Red earthenware resembling the Etruscan, and found in the ruins of Gergovia near Clermont.

Crucibles, &c.

	Hessian. (i.)	Paris. (ii.)	English. (iii.)	St. Etienne. (iv.)	Nemours. (v.)	Bohemia. (vi.)	Le Creusot. (vii.)
Silica	0.709	0.646	0.637	0.652	0.674	0.680	0.680
Alumina	0.218	0.344	0.207	0.250	0.320	0.290	0.280
Oxide of Iron	0.038	0.010	0.040	0.072	0.008	0.022	0.020
Magnesia	trace	..	..	trace	trace	0.005	trace
Water	..	..	0.103	..	..	..	0.010
	0.995	1.000	0.987	0.974	1.002	0.997	0.990

(i.) Hessian crucibles—formed of a clay very aluminous, with which siliceous sand is mixed. They sustain rapid changes of temperature without fracture, but cannot retain fused litharge very long together, and have too coarse a grain for many purposes.

(ii). Paris crucibles, manufactured by Beaufaye—they are made from the clay of Andennes, near Namur; part of the material being baked and coarsely powdered, and the rest in its natural state: no sand is mixed with it, and the inner surface of the vessels is finished with a thin coat of the unbaked material. They are said to be more refractory than the Hessian vessels, not more liable to fly by change of temperature, and more retentive of litharge.

(iii.) Fragment of an unbaked crucible prepared for an English cast-steel work.

(iv.) Paste with which the crucibles are made for the steel works of Berardière, near St. Etienne.

(v.) Fragment of a used crucible from the glass works of Bagneaux, near Nemours; it had been made from the clay of Forges (Seine Inférieure).

(vi.) A used crucible from a Bohemian glass-house.

(vii.) Bricks with which the blast furnaces at Creusot are con-

structed; they are made of a mixture of baked and unbaked clay. —*Annales de Chimie*, i. 469.

28. *On the Composition of simple Alimentary Substances, by* Dr. Prout.—It is well known that Dr. Prout has of late years devoted that portion of his attention which he gives to chemistry, exclusively to the consideration of organized substances, with the important object of making the knowledge he might obtain subservient to the study of physiology and pathology; and during the last session of the Royal Society, a paper by this philosopher was read, containing many important and apparently accurate results relative to the particular subjects which he has pursued; some account of which we are desirous of giving in this place.

Dr. Prout's first object was to devise, if possible, an unexceptionable mode of determining the proportions of the three or four principles, which, with few exceptions, form organic bodies; and after numerous trials, he adopted a method founded upon the following well known principles. When an organic product, containing three elements, hydrogen, carbon, and oxygen, is burnt in oxygen gas, one of three things must happen: i. The original bulk of oxygen gas may remain the same, in which case the hydrogen and oxygen in the substance must exist in it in the same proportions in which they exist in water; or, ii. The original bulk of the oxygen may be increased, in which case the oxygen must exist in the substance in a greater proportion than it exists in water; or, iii. The original bulk of the oxygen gas may be diminished; in which case the hydrogen must predominate. Hence it is obvious, that, in the first of these cases, the composition of a substance may be determined, by simply ascertaining the quantity of carbonic acid gas yielded by a known quantity of it; while, in the other two, the same can be readily ascertained by means of the same data, and by noting the excess or diminution of the original bulk of the oxygen gas employed.

The apparatus consists of two inverted glass syphons which act the part of gasometers; these are connected when required, by a small green glass tube, in which the substance is to be decomposed and burnt: the syphons are very carefully gradated; so that the quantity of gas in them can be accurately estimated; and are supplied with cocks both above and below, so that they can be filled with mercury, the mercury drawn off and gas introduced, the gas transferred through the green glass tube, or the contents retained in an undisturbed state, with the utmost readiness and ease. The substance to be decomposed, may be put into a platina tray, and introduced alone into the green glass tube, and being there heated by a spirit lamp, be burnt in the gas passing over it; or it may be mixed with pure siliceous sand; or, what is most generally preferable, be mixed with peroxide of copper, which is always left, in consequence of the excess of oxygen gas used, in the state in which it was introduced. After the experiment the volume of gas is easily

corrected for pressure, and if necessary for temperature, and the carbonic acid ascertained by the removal and analysis of a portion. No correction is required for moisture, the gas always being used saturated with water.

Dr. Prout considers the principal alimentary substances as reducible to three great classes, the *saccharine*, the *oily*, and the *albuminous;* and his paper relates to the first of these. This, with certain exceptions, includes the substances in which, according to MM. Gay Lussac and Thenard, the oxygen and hydrogen are in the same proportion as in water. Such substances are principally derived from the vegetable kingdom, and being at the same time *alimentary*, Dr. Prout uses the terms *saccharine principle* and *vegetable aliment* as synonymous.

The following tables show some of Dr. Prout's results with several substances, extreme care having been taken in every case to obtain the bodies pure, and new processes often resorted to for that purpose.

Sugar.

	Carbon.	Water.
Pure sugar-candy..........	42.85	57.15
Impure sugar-candy........	41.5 to 42.5	58.5 to 57.5
East India sugar-candy......	41.9	58.1
English refined sugar.......	41.5 to 42.5	58.5 to 57.5
East India refined sugar.....	42.2	57.8
Maple sugar...............	42.1	57.9
Beet root sugar............	42.1	57.9
East India moist sugar......	40.88	59.12
Sugar of diabetic urine......	36. to 40?	64. to 60?
Sugar of Narbonne honey.....	36.36	63.63
Sugar from starch..........	36.2	63.8

Amylaceous Principle.

	Carbon.	Water.
Fine wheat starch...............	37.5	62.5
„ dried (i.)........	42.8	57.2
„ highly dried (ii.)...	44	56
Arrow root......................	36.4	63.6
„ dried (iii.)..............	42.8	57.2
„ highly dried (iv.).......	44.4	55.6

(i.) Dried between 200° and 212° for twenty hours, lost 12.5 per cent.

(ii.) Part of the former, dried between 300° and 350° for six hours, lost 2.3 per cent.

(iii.) Dried as (i.), lost 15 per cent.

(iv.) Part of the last, heated to 212° for six hours longer, lost 3.2 per cent. more.

Lignin, or Woody Fibre,

Obtained by rasping wood, and then pulverising it in a mortar; boiling the impalpable powder in water till nothing more was

removed, then in alcohol; again in water, and dried in the air till they ceased to lose weight.

	Carbon.	Water.
From box....................	42.7	57.3
" dried (i.)............	50.	50.
From willow..................	42.6	57.4
" dried (i.).........	49.8	50.2

(i.) Dried at 212° for six hours, afterwards between 300° and 350° for six hours. That from box lost 14.6, that from willow 14.4 per cent.

Acetic acid....................	47.05	52.95
Sugar of milk.................	40.	60.
Manna sugar..................	38.7	61.3
Gum arabic....................	36.3	63.7
" dried (i.)...........	41.4	58.6

(i.) Dried between 200° and 212° for twenty hours, lost 12.4 per cent. The same gum further heated to between 300° and 350° for six hours, lost only 2.6 per cent., and had become deep brown.

Vegetable Acids.	Carbon.	Water.	Oxygen.
Oxalic acid............	19.04	42.85	38.11
Citric acid.............	34.28	42.85	22.87
Tartaric acid...........	32.00	36.00	32.00
Malic acid............	40.68	45.76	13.56
Saclactic acid..........	33.33	44.44	22.22

29. *Preparation of Sulphate of Quinia and Kinic Acid, without the use of Alcohol.*—The following is the process of MM. Henry and Plisson: About two pounds of bark are to be coarsely powdered and boiled with water, acidulated with sulphuric acid in the usual manner. When the hot liquors are cleared, recently prepared and moist hydrate of lead is to be added until the fluid is neutral, and has acquired a faint yellow colour; this must be done carefully, lest too much hydrate of lead be added. As the decoloration of the decoction is necessary, the liquid, if it remains turbid until the next morning, must have a little more hydrate added and be re-filtered, but the operation is rarely subject to this inconvenience, being usually finished in a few hours. The yellow liquid contains a little kinate of lead, much kinate of lime, kinate of quinia or cinchonia, a little colouring matter, and traces of other substances. The washed deposite consists of colouring matter, combined with oxide of lead, sulphate of lead, and a portion of free quinia; contains no sub-kinate of lead.

The lead, dissolved in the fluid, is to be separated by a few drops of sulphuric acid, or a small current of sulphuretted hydrogen, and the filtered liquid is to be precipitated by adding caustic lime, previously mixed into a thin paste with water, until the earth is in very slight excess; in this manner the quinia is precipitated. The addition of sulphuric acid readily converts this [illegible] sulphate,

which may be obtained in very white and silky crystals. The fluid left after the separation of the quinia, contains a kinate of lime almost pure. Being evaporated until of the consistence of syrup, it readily crystallizes in a mass, which may then be purified by recrystallization. The kinate of lime may be precipitated by means of alcohol, and then be crystallized after solution in water or diluted alcohol; or, by adding oxalic acid drop by drop, according to the directions of M. Vauquelin, the lime may be separated and kinic acid obtained. Two thirds of the quinia or cinchonia in a specimen of bark may be thus separated, and with such facility as to offer a ready test of the presence of these alkalies in any wood or bark submitted to examination.—*Ann. de Chimie*, xxxv., 166.

30. *Pure Narcotine prepared.*—The following process is that practised by Mr. Carpenter. Digest one ounce of coarsely powdered opium in one pint of ether for ten days, frequently submitting it to ebullition in a water bath; separate the ether and add fresh portions until the opium is exhausted; place the ethereal solution in a wide-mouthed bottle, and, covering the mouth with bibulous paper, allow the ether to evaporate spontaneously, but slowly; as the fluid diminishes, it leaves the sides of the bottle coated with crystals of narcotine; as the solution becomes more dense, the crystals enlarge and accumulate, and the bottom of the vessel is covered with large transparent crystals, accompanied with a brown viscid liquor and extract, which contains an acid resin, caoutchouc, &c. Separate these substances and wash the crystals in successive portions of cold ether to remove the extract; then dissolve them in warm ether, and evaporate slowly as before; beautiful snow white crystals of pure narcotine will be obtained: those on the sides of the vessel assume plumose and arborescent forms; they enlarge as the solution becomes more concentrated, and the bottom of the bottle becomes covered with pure narcotine, assuming the rhomboidal prismatic form with some modifications of macled crystals. The crystals towards the bottom are transparent, but the most minute at the top are opaque and snow white. By picking out the largest and most regular crystals, again dissolving and evaporating, and repeating the same process, each time selecting the largest and best crystals, some were obtained the eighth of an inch in diameter, and still larger might be produced by similar operations.—*Silliman's Jour.*, xiii. 27.

31. *Uncertain Nature of Jalapia.*—Relative to Mr. Hume's supposed vegeto-alkali *Jalapia*, M. Pelletier says it is nothing more than a mixture of sulphate of lime and sulphate of ammonia.—*Jour. de Pharmacie.*

32. *Preparation of pure Mellitic Acid, by* M. Wöhler.—Concentrated solution of carbonate of ammonia was poured upon finely pulverised mellite, and boiled until the excess of ammonia was

dissipated; the solution was filtered and left to crystallize. The pure crystals, being dissolved in water, were precipitated by acetate of lead, and the mellitate of lead, after being well washed was decomposed by sulphuretted hydrogen; being filtered, the solution was evaporated to dryness, during which the mellitic acid precipitated as a white powder; being dissolved in cold alcohol, and left to evaporate spontaneously, the acid was obtained in acicular crystals. In this state it is very acid, unaltered by air, very soluble in water and alcohol, and sustains a considerable heat without change; it does not fuze, but ultimately sublimes, though probably not without decomposition. When boiled for a considerable time with alcohol, it undergoes a peculiar change, and occasions the production of a new acid substance, resembling the benzoic acid.

33. *On a New Acid existing in Iceland Moss.*—The reddish purple colour which is produced by adding a decoction of Iceland moss to per-salts of iron, has been attributed to the presence of gallic acid, but is found by M. Pfaff to be occasioned by a new acid body which may be separated in tha following manner. A pound of the lichen cut small is to be macerated in solution of carbonate of potassa, until all that is soluble is separated; the above quantity will neutralize two gros* of the carbonate. The filtered liquor is to be precipitated by acetate of lead, and the brown precipitate produced, when well washed, is to be diffused through water, and sulphuretted hydrogen passed through it until all the lead is separated. The filtered liquor is acid, and by spontaneous evaporation, yields dendritic crystals. The crystals, when heated, carbonize, but produce no odour like that of tartaric acid, and lime is left. If they be dissolved and acted upon by alkaline carbonates, carbonate of lime is thrown down, and alkaline salts, containing the new acid, are produced.

The potash salt crystallizes in quadrilateral prisms, needles or plates, and is not deliquescent. The soda salt has similar characters, and the ammonia salt crystallizes in needles. These salts abundantly precipitate the acetate and muriate of iron of a red brown colour; they precipitate sulphate and nitrate of zinc white; muriate of manganese slightly of a clear brown colour; barytic and strontian salts abundantly white; being mixed with strong solutions of muriate or acetate of lime, they gradually produce an acicular crystalline white precipitate; acetate of silver yields an abundant white precipitate, which does not change colour in less than twenty-four hours: they do not precipitate salts of glucina, magnesia, alumine, uranium, nickel, copper, cobalt, gold or platina. This substance has been named the lichenic acid, and is distinguished from boletic acid by the different character of its vapour, and by forming an insoluble salt with baryta.—*Bull. Univ.* A. viii. 270.

34. *Remarks on the Preparation of M. Gautier's Ferro-prussiate*

* About one hundred and twenty grains.

of Potash, as described in this Journal for July, 1827.*—It is stated in the above article, "numerous investigations induced M. Gautier to conclude that when animal matter is calcined alone, it yields but little cyanogen; that when mixed with potash it gives more; that the *substitution* of *nitre* for potash, and the addition of iron or scales of iron, augmented the production of cyanogen and gave a ferro-prussiate. The following is the process of manufacture to which M. Gautier has ultimately arrived," (for which see the Journal, 227.)

M. Gautier giving the proportions of materials, directs—

Blood in a dry state . . . . 3 parts
Nitre . . . . 1 ,,
Iron scales . . . . $\frac{1}{50}$ of the blood employed.

Blood not being at hand, animal muscular fibre was substituted, and the following results were obtained. I am not aware that the dried parts of animal muscular fibre are more inflammable than the coagulated and dried parts of blood:—

Muscular fibre . . . . 3 parts
Nitre . . . . 1 ,,
Iron filings . . . . . . $\frac{1}{50}$ of the undried muscle employed.

The muscular fibre, nitre and iron filings were beat into a mass, and partially dried by a moderate heat; they were then returned to the mortar and reduced to a perfectly homogeneous greyish white powder. This was dried and weighed, and appeared to be reduced to nearly equal parts of nitrate of potash and animal fibre.

The desiccation having been completed by a very moderate heat on a sand bath, will not, as far as I am aware, differ materially from that produced by exposing the mass in "an airy situation to dry," as nitrate of potash undergoes no decomposition by admixture with animal matters at a low temperature.

When the desiccation was completed, the mixture was charged into an iron cylinder, placed in the sand-bath, and though combustion was not anticipated in this part of the process, yet the mouth of the cylinder was turned towards the wall, lest an accident should occur, (which appeared to me to be more than probable in some stage of the process.) In about two hours after the cylinder had been heated, I was surprised to see its contents ejected with considerable force, in a state of brilliant combustion. Supposing something in the above experiment had been overlooked, and that, if the materials had been longer in contact previously to subjecting them to complete desiccation, this inflammation would not have taken place, the experiment was repeated with the following precautions: after the muscular fibre had been subjected to the action of the pestle in combination with the prescribed quantity of nitrate of potash, the mass was boiled with water for some hours, and then gently evaporated to dryness; even now, by applying a piece of red-hot charcoal, it was found that the nitre was in a condition to enter

* Pages 207 and 208.

into active combustion, and if the cylinder had been again charged and subjected to a temperature capable of producing ignition, there cannot be a doubt, but that a similar inflammation would have taken place.

However this might be, this quantity of material was now mixed with hydrate of potash to an equal weight with the nitre used; and the mass subjected to the heat of a sand-bath for some hours, and afterwards submitted to the action of a naked fire for rather more than an hour, and the heat brought up to redness. No considerable action took place, but some particles of the carbonaceous matter were ejected, and produced brilliant scintillations in the fire, so that we may conclude, notwithstanding the presence of so large a quantity of potash, the properties of the nitre were not destroyed.

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III. Natural History.

1. *Squalls of Wind on the African Shores.*—The following description is by D. M. Milnegraden, from the relations of his father. "The approach of the squall is generally foreboded by the appearance of jet black clouds over the land, moving in a direction towards the sea, at the same time that a gentle breeze blows towards the shore. In these circumstances, the precaution which my father usually adopted, was to take in immediately all sail, so as to leave the ship under bare poles, and send the whole of the crew below decks. As the tornado approaches nearer, the rain is observed to be gushing down in torrents, and the lightning darting down from the clouds with such profusion, as to resemble continued showers of electric matter. When, however, the squall comes within the distance of about half a mile from the ship, these electric appearances altogether cease; the rain only continues in the same manner. As the tornado is passing over the ship, a loud crackling noise is distinctly heard among the rigging, occasioned by the electric matter streaming down the masts, whose points serve to attract it, and I think that I have been told, that when this phenomenon takes place at night, a glimmering of light is observed over every part of the rigging. But when the squall has removed to about half a mile beyond the ship, exactly the same appearances return by which the squall was characterised in coming off the shore, and before reaching the same distance from the ship. The lightning is again seen to be descending in continued sheets and in such abundance as even to resemble the torrents of rain themselves which accompany the squall. These squalls take place every day during a certain season of the year called the Harmattan season. The jet black clouds begin to appear moving from the mountains about nine in the morning, and reach the sea about [illegible] in the afternoon. Another very singular fact attending these tornados is, that, after they have moved out eight or nine leagues to sea, where

they become apparently expended, the lightning is seen to rise up from the sea. The violence of the wind during the continuance of the storm is excessive."—*Jameson's Journal*, 1823, p. 367.

2. *Destruction of an Oak by Lightning.*—M. Muncke describes a case in which an oak, being struck by lightning, was rent and destroyed in an extraordinary manner. The trunk of the tree was about fifteen feet in height, a foot and a half or two feet in diameter at the branches, and three feet in diameter at the root. The top of the tree was separated as if by the stroke of a hatchet, and without any appearance of carbonization: the trunk was torn into a thousand pieces, exceedingly small in size when compared with the origiual mass, and thrown to a great distance. The division and destruction was such as to sustain the thought, that in certain cases the lightning might cause the entire dispersion of the tree, an opinion which was suggested by the circumstance that lightning which had fallen at Le Chateau de Marbourg left no traces of a rafter that had occurred in its course.—*Bull. Univ.* A. viii. 194.

3. *Description of a Meteoric Fire-Ball seen at New Haven by the Rev. S. E. Dwight.*—The meteor appeared on Saturday evening, March 21, 1813, a little before ten o'clock. The sky was much overcast, but the covering thin, and the stars were in full view towards the north where the meteor appeared. Dr. Dwight was standing on a platform on the north side of the house looking eastward, when the light first broke upon him, and for a moment supposed it to be lightning, but was instantly induced by its continuance to look at the luminary. The following are the observations made at the time.

i. The meteor was at first about 35° above the horizon, and, judging from the course of a fence near at hand, its direction about N. 20°. E.

ii. Its figure nearly that of an ellipse, with the ends in a slight degree sharpened or angular.

iii. The length of its transverse diameter appeared to be about equal to the apparent diameter of the moon when on the meridian, and that of the conjugate about three fourths of the transverse.

iv. The colour rather more yellow than that of the moon.

v. A tail of light, ten or twelve degrees in length, was formed behind it; broadest near the body; decreasing in breadth very slowly for about two-fifths of its length, after which it was uniform, and about as wide as the apparent diameter of Venus. The direction of the tail was coincident with that of the transverse diameter.

vi. The ball was far more luminous than the tail, and the part connected with the tail scarcely less distinct than the opposite part.

vii. The light was such that all objects cast distinct shadows, though less strongly marked than when the moon is full.

viii. Numerous sparks continually passed from the ball of the

meteor; they were of the apparent size, but much more brilliant than the smaller stars, and after descending a little distance, disappeared.

ix. The meteor was visible for about eight or perhaps ten seconds.

x. A second or two before its disappearance, three much larger sparks or luminous fragments were thrown off at once, two of them the apparent size of Venus, the third larger. These were the last pieces which were seen to leave the body. Their paths were at first nearly parallel with that of the meteor, yet beneath it. From this direction, however, they all deviated constantly and rapidly, in parabolic curves, until they seemed falling perpendicularly towards the earth. Each fragment became less and less distinct until it disappeared. The largest continued visible until about 20° from the horizon.

xi. The meteor itself disappeared as suddenly as if, in one indivisible moment, it had passed into a medium absolutely opaque, or as if, at a given moment, it had left the atmosphere; but a few moments afterwards there was a distinct and somewhat extensive illumination over that part of the sky for about a second.

xii. When the meteor disappeared, it was about 30° above the horizon in the direction of N. 45° E. or 25° east of the place where it was first seen. The direction of the path was probably from W. by S. to E. by N. The meteor was obviously going from the observer, its path making an angle with the optic axis of about 60°.

xiii. Between eight and ten minutes after the disappearance of the meteor, there was a loud and heavy report, accompanied by a very sensible jar; it did not much resemble either thunder or the report of a cannon, but was louder, shorter, and sharper than either, and was followed by no perceptible echo.

xiv. A friend of Dr. Dwight's, who was in Berlin at the time, about twenty-three miles due N. of Newhaven, saw the meteor distinctly, but made no particular observations. His account accorded generally with that given; but the meteor appeared to him larger, more elevated, and somewhat more to the east in its apparent place. No account could be obtained of any fragments which had fallen from it.—*Silliman's Journal*, xiii. 35.

4. *Remarkable Meteoric Phenomenon, described by* Chladni.—A noise, resembling thunder in its rolling nature, was heard at Saarbruck and the environs, about four o'clock on the 1st of April, 1826, the atmosphere being clear, and the sun shining brightly. During the sound, a greyish object, apparently about three feet and a half in height, was seen in the air, rapidly approaching the earth, and there expanding itself like a sheet; there was then silence for about a minute, after which another sound, resembling thunder, was heard, as if it had originated at the place where the meteor fell. Nothing was found when the place was afterwards examined.—*Bull. Univ.* A. viii. 148.

5. *Aurora Borealis seen in the Day-time at Canonmills.*—The morning of Sunday, September 9th, was rainy, with a light gale from the N.E. Before mid-day the wind began to veer to the west, and the clouds in the north-western horizon cleared away: the blue sky in that quarter assumed the form of the segment of a very large circle, with a well-defined line, the clouds above continuing dense, and covering the rest of the heavens. The centre of the azure arch gradually inclined more to the north, and reached an elevation of nearly 20°. In a short time very thin fleecy clouds began to rise from the horizon within the blue arch; and through these very faint perpendicular streaks, of a sort of milky light, could be perceived shooting; the eye being thus guided, could likewise detect the same pale streaks passing over the intense azure arch, but they were extremely slight and evanescent. Between nine and ten in the evening of the same day, the aurora borealis was very brilliant: so that there is no reason to doubt that the azure arch in the morning, and the pale light seen shooting across it, were connected with the same phenomenon.—*Jameson's Jour.* 1827, p. 378.

6. *Aurora Borealis in Siberia.*—Baron Wrangle says, that in Siberia, when shooting stars pass across the space occupied by polar lights, fiery beams suddenly arise in the place traversed by the shooting star: further, that when a polar beam rises high towards the zenith, the full moon also being high, it gradually forms a luminous circle around the moon, at a distance of 20° or 30° from her, remains in this form for a short time, and then disappears.

7. *On the Presence of Ammonia in Argillaceous Minerals.*—Being engaged in the examination of different specimens of gypsum, M. Bouis observed, that traces of ammonia were evident in one containing much argillaceous matter. The peculiar odour common to argillaceous minerals when breathed upon, was very striking in this specimen of gypsum; when a portion of it was moistened with solution of potash, and muriatic acid brought near, white vapours were produced, and reddened litmus paper was very quickly rendered of a blue colour in its vicinity.

It was now suspected that all mineral substances, emitting an argillaceous odour, contained ammonia; a great number of specimens were tried, being moistened with solution of caustic potash, and examined by litmus paper. In no case was ammonia absent, and with common clay it continued to be evolved for more than two days. Amongst the substances tried, were pipe clay, other clays, numerous gypsums, Paris plaster, steatite, &c. The antiquity of the mineral seemed to have no relation to the ammonia.

M. Bouis concludes that, in all cases, the argillaceous smell of minerals is connected with, and dependent upon, the presence of ammonia, the latter being the vehicle of this particular odour.—*Annales de Chimie*, xxxv. 333.

8. *Composition of Apatite.*—According to M. Rose, the apatite from the following localities gave the annexed proportions of chloride and fluoride of calcium, the rest being phosphate of lime with occasional traces of iron and magnesia:—

	S. G.	Chlo. Calc.	Fluor. Calc.
Apatite from Suarum in Norway	3.174	4.280	4.590
Cabo de Gota in Spain .	3.235	0.885	7.049
Arendal	3.194	0.801	7.010
Greiner in the Tyrol . .	3.175	0.150	7.690
Faldigl, ditto . . .	3.166	0.100	7.620
St. Gothard . . .	3.197	trace	7.690
Ehrenfriedersdorf . .	3.211	trace	7.690

Annales de Chimie.

9. *Burmese Petroleum Wells.*—"The gentlemen of the mission examined carefully the celebrated Petroleum Wells, near which they remained for eight days, owing to the accident of the steam-vessel taking the ground in their vicinity. Some of the wells are from thirty-seven to fifty-three fathoms in depth, and are said to yield at an average, daily, from 130 to 185 gallons of the earth-oil. The wells are scattered over an area of about sixteen square miles. The wells are private property, the owners paying a tax of five per cent. of the produce to the state. This commodity is almost universally used by the Burmans as lamp oil. Its price on the spot does not, on an average, exceed from fivepence to sevenpence halfpenny per cwt. The other useful mineral or saline productions of the Burman empire are coal, saltpetre, soda, and culinary salt. One of the lakes affording the latter, which is within six or seven miles of the capital, was examined by the gentlemen of the mission." Crawford's Mission to Ava.—*Jameson's Journal*, 1827, p. 366.

10. *Direction of the Branches of Trees.*—Professor Eaton remarks that all trees with spreading branches accommodate the direction of the lower branches to the surface of the earth over which they extend, as may be seen in orchards growing on the sides of hills, and in all open forests; and inquires what influence can the earth have upon the branches on the upper side of a tree, which causes them to form a different angle with the body of the tree from the angle formed by the branches on the lower side, so that all the branches hold a parallel direction to the earth's surface.—*Silliman's Journal*, xiii. 194.

11. *Effects of Light on Vegetation.*—The following observations by Professor Eaton are dated Rensselaer school, Troy, April [illegible], 1827. "Clouds and rain have obscured the hemisphere during the last six days. In that time the leaves of all the forests which are seen from this place have greatly expanded. But they were all of a pallid hue until this afternoon. Within the period of about six hours, they have all changed their colour to a beautiful green. As the only efficient change which has taken place, is that we have a

serene sky, and a bright sun, we may say with confidence that this change of colour is produced by the action of the sun's rays.

Seven years ago, next month, I had a still more favourable opportunity to observe this phenomenon in company with the Hon. J. Lansing, late Chancellor of this State. While we were engaged in taking a geological survey of his manor of Blenheim, the leaves of the forest had expanded to almost the common size in cloudy weather. I believe the sun had scarcely shone upon them in twenty days. Standing upon a hill, we observed that the dense forests upon the opposite side of the Schoharie were almost white. The sun now began to shine in full brightness. The colour of the forest absolutely changed so fast that we could perceive its progress. By the middle of the afternoon, the whole of these extensive forests, many miles in length, presented their usual summer dress.—*Silliman's Journal*, xiii. 193.

12. *Organization and Reproduction of the Trufle.*—The trufle, according to the account given of it by M. Turpin, in a memoir read to the Academy of Sciences, is a vegetable entirely destitute of leafy appendages or of roots; it is nothing more than a rounded subterraneous mass, absorbing nourishment upon every point of its surface, and the reproduction of which is dependent upon bodies generated within its substance. The trufle is composed of, i. globular vesicles, destined, to the reproduction of the vegetable; ii. short and barren filaments, called by M. Turpin, *tigellules*. The whole forms a substance, at first white, but which becomes brown by age, with the exception of particular white veins. This change of colour is dependent upon the presence of the reproductive bodies or *trufinelles*. Each globular vesicle is fitted to give birth, on its internal surface, to a multitude of these reproductive bodies, but there are only a few of them which perfect the young vegetable. These dilate considerably, and produce internally other smaller vesicles, of which, two, three, or four increase in size, become brown, are beset with small points on their exterior surface, and fill the interior of the larger vesicle. The small masses thus formed, are the *trufinelles*, and become trufles after the death of their parent. Thus the brown parts of the trufle are those which contain the trufinelles, and the interposed white veins are the parts which are destitute of trufinelles. The parent trufle, having accomplished its growth and the formation of the reproductive bodies within, gradually dissolves and supplies that aliment to the young vegetable which is proper for them; the cavity originally occupied by it in the earth is then left occupied by a multitude of young trufles, of which the stronger starve or destroy the others, whilst they frequently adhere together, and, enlarging in size, reproduce the phenomena already described.

The reporters of this memoir to the Academy state that they have verified M. Turpin's [illegible] a circumstance in the natural history [illegible]plained. If the

method described be the only mode in which the trufle is reproduced, then it is difficult to comprehend the enormous multiplication of that vegetable in certain parts of France, where immense quantities are annually collected without exhausting or even diminishing the race. If the plant has no means of progression, how can the young trufles leave the place of their birth, and become disseminated over the soil? The Mémoire received the approbation of the Academy.—*Revue Ency.* xxxv. 794.

13. *Alteration of Corn in a subterraneous Repository.*—An inhabitant of Deneuvre in the department of Meurthe, whilst excavating in the locality of the ancient citadel of that town, found a large quantity of corn which appeared to have b[illegible] [illegible]onized. A portion was sent to M. Braconnot for examina[illegible] [illegible] any particulars of the cavity containing it. The grain wa[illegible] [illegible]ooth on the exterior, and unchanged in form, bu[illegible] [illegible] aspect ann[illegible]nced the entire destruction of its proximate p[illegible]nciples. It floated [illegible] water, could be crushed between the finger to a black powder, and when rubbed on paper left traces resembling those of black chalk.

Being analysed, it was found to consist principally of a substance resembling ulmine in its properties, ulmate of lime and carbonaceous matter: the proportions were

Ulmine	[illegible].5
Ulmate of lime, containing some phosphate of lime and a little oxide of iron	[illegible].0
Carbonaceous matter	30.0
Muriates of potash and lime / Nitrates of potash and lime	1.5
Fatty matter of the consistency of wax, undetermined.	
	100.0

Although the time during which this corn has been stored up is probably very long, still M. Braconnot thinks the principal cause of the change in it has been humidity; and thinks also that the same may have been the case with the corn lately found in an Egyptian tomb*, and quotes the known fact of corn having been found at Scarpone, an ancient Roman station, preserved in good condition during eighteen centuries, in a reservoir constructed of Roman mortar.

The best use that could be made of the carbonized corn of Deneuvre was to apply it as a manure, for it contained the best elements of a substance of this kind, and M. Braconnot had long since observed the presence of ulmine in good manure, its acid properties, and its effects on vegetation. He adds also that Bruyères earth of excellent quality gave one-fourth of a combustible matter formed of ulmine and a carbonaceous body but little soluble in potash, the remaining three-fourths being a pure siliceous sand without a trace of lime. Yet so effectual is this earth, that, where it cannot be obtained, certain exotics cannot be cultivated.—*Annales de Chimie*, xxxv. 262.

* See p. 910 of the last Number.

14. *Quick Method of putting Insects to Death.*—The following method is by M. Ricord, for the use of naturalists. The insect is to be fixed on a piece of cork and put under a jar or vessel with a little ether; the latter being placed either in a capsule, or on the plate on which the jar or glass is placed: the vessel should apply closely, that the vapour of the ether may be retained, and the air within be prevented from changing its place. The insect thus immersed in the ethereal atmosphere will soon die without having time to hurt its form or appearance by violence.—*Bull. Univ.* B. vii. 29[illegible]

15. *Destruction of Snails by common Salt.*—M. Em. Rousseau had applied common salt as a manure to a small piece of garden, and remarked that [illegible] the snails had come in contact with the salt they quickly die[illegible] [illegible]hing [illegible] confirm the fact, he strewed some salt upon the ground and placed a number of snails amongst it; all those which came out of their shell and touched the salt immediately threw out a greenish globula froth, and in a few minutes were dead. The fact may be turned to account by agriculturists and gardeners.—*Bull. Univ.* D. vii 276.

16. *Remarkable Hairy Man.*—The following account is given of an individual of this kind in Crawford's Mission to Ava. "As connected with this department may be mentioned the existence at Ava of a man covered from head to foot with hair, whose history is not less remarkable than that of the celebrated porcupine man who excited so much curiosity in England and other parts of Europe near a century ago. The hair on the face of this singular being, the ears included, is shaggy, and about eight inches long. On the breast and shoulders it is from four to five. It is singular that the teeth of this individual are defective in number, the molares or grinders being entirely wanting. This person is a native of the Shan country, or Lao, and from the banks of the upper portion of the Saluen, or Martaban river; he was presented to the king of Ava as a curiosity, by the prince of that country. At Ava he married a Burmese woman, by whom he has two daughters; the eldest resembles her mother, the youngest is covered with hair like her father, only that it is white or fair, whereas his is now brown or black, having, however, been fair when a child, like that of the infant. With the exceptions mentioned, both the father and his child are perfectly well-formed, and, indeed, for the Burman race, rather handsome. The whole family were sent by the king to the residence of the Mission, where drawings and descriptions of them were taken."—*Jameson's Jour.* 1827, p. 368.

17. *Application of Remedies by Absorption from the Surface.*—The following are the results obtained by M. Bailly, who has been assiduously engaged in trying this plan.

Salts of Morphia, applied in this manner, speedily exhibit their

action upon the brain and nervous system, by the contraction of the pupils, and often by dysuria and ischuria; nausea and vomiting are rare; sometimes a sensation of itching is felt in the nasal cavities, and papular eruptions not unfrequently appear upon the skin.

Extract of Belladonna, applied upon the upper surface of the feet, produced all the consequences derived from its internal exhibition; such as dilatation of the pupil and impaired vision.

Extract of Squill, while it augments transpiration, promotes the urinary secretion, and facilitates expectoration.

Well powdered Strychnine supports the suppuration of wounds tolerably well, and stimulates the locomotive system without inconveniently exciting the brain. It happens also in certain palsies, such as those which are caused by the carbonate of lead, that the power of motion is restored without the production of those violent shocks which have been so unpleasant to patients. M. Bailly has observed, with respect to this medicine in general, that it often excites a marked turgescence about the head, heightening the colour of the face, which demands the suspension of the remedy, if not the intervention of blood-letting.

Perchloride of Mercury (corrosive sublimate) produces an intense sensation of heat, and corrodes the parts with which it comes in contact. Sometimes, however, it has been known to relieve the pains of exostoses, &c. *The proto-chloride* (calomel) also excites pain, particularly if rubbed upon a recently blistered surface. In this way it may cure old syphilitic affections; but as a set-off against these advantages, there is sometimes a difficulty in keeping up the action, as the absorbent powers of the surface wear out by long continued contact.

One great advantage of the *endermic* practice is the exemption of the digestive organs from an inconvenient or unaccustomed stimulus; and its importance must be apparent where the stomach is incapable of retaining medicines, or the power of deglutition is lost.—*Nouv. Bib. Med.—Med. Rep.* v. 341.

18. *On the Strix Cunicularia, or Coquimbo Owl.*—Captain Head, and every reader of his "Rough Notes," will, we are sure thank us for any hint tending to throw light on facts related in that spirited and interesting narrative; particularly as, in the course of his adventures, circumstances are occasionally recorded somewhat startling to those who are in the habit of considering whatever surpasses their ken or comprehension as a travellers' tale. Thus the concluding part of the following passage, however true to the very letter, as we shall show, has we know excited considerable surprise, and possibly considerable doubt as to its accuracy.

"The Biscacho* is found all over the plains of the Pampas; like rabbits they live in holes, which are in groups in every direc-

* This animal is probably either the Cavia Paca, Spotted Cavy, or Arctomys Monax, Ferruginous Brown Marmot, though the latter is described as principally found in North America.

tion, and which make galloping over these plains very dangerous. These animals are never seen in the day, but as soon as the lower limb of the sun reaches the horizon, they are seen issuing from their holes in all directions, which are scattered in groups like little villages, all over the Pampas. The biscachos, when full grown, are nearly as large as badgers, but their head resembles a rabbit, excepting that they have large bushy whiskers. In the evening they sit outside their holes, and they all appear to be moralising. They are the most serious looking animals I ever saw; and even the young ones are grey headed, have mustachios, and look thoughtful and grave. *In the day time their holes are always guarded by two little owls, who are never an instant away from their post. As one gallops by these owls, they always stand looking at the stranger and then at each other, moving their old-fashioned heads in a manner which is quite ridiculous, until one rushes by them, when fear gets the better of their dignified looks, and they both run into the biscachos' hole.*"—(Head's Rough Notes, p. 82.)

Captain Head has not given us the name of this owl, but in all probability it was the Strix Cunicularia, or Coquimbo Owl, which is described as flying *in pairs*, sometimes by day, and making its nest *in long subterraneous burrows**. In the singular motion of its head, it however corresponds with the Strix Brasiliana, or Brownish Horned Owl, mentioned by Marcgrave in his History of Brazil, which he says is easily tamed, and can so *turn about its neck* that the tip of the beak shall exactly point at the middle of the back; that it also plays with men like an ape, *making many mowes*, (as Willoughby translates it,) *and antic mimical faces*, and snapping with its bill. But for the best account we have met with, we are indebted to the splendid continuation of Wilson's American Ornithology by Lucien Bonaparte, under the title "Burrowing Owl—a bird," he says, "that so far from seeking refuge in the ruined habitations of man, fixes his residence within the earth; instead of concealing itself in solitary recesses of the forests, delights to dwell on open plains, in company with animals remarkable for their social disposition, neatness, and order. Instead of sailing heavily forth in the obscurity of the evening or morning twilight, and then retreating to its secluded abode, this bird enjoys the broadest glare of the noon-day sun, and flying rapidly along, searches for food or pleasure during the cheerful light of the day. In the trans-Mississippian territories of the United States, this very singular bird *resides exclusively in the villages of the Marmot, or Prairie Dog*, whose excavations are so commodious, as to render it unnecessary that it should dig for itself, as it is said to do in other parts of the world, where no burrowing animals exist. These villages are very numerous, and variable in their extent, sometimes covering only a few acres, and at others spreading over the surface of the country for miles together. They are composed of slightly

* Turton, Lin. vol. i. 169.

elevated mounds, about two feet in width at the base, and seldom exceeding eighteen inches in height. In all these Prairie dog villages, the burrowing owl is seen moving briskly about, or else in small flocks scattered among the mounds, and at a distance it may be mistaken for the marmot itself when sitting erect. They manifest *but little timidity, and allow themselves to be approached sufficiently close for shooting*; but if alarmed, some or all of them soar away, and settle down again at a short distance: if further disturbed, their flight is continued until they are no longer in view, *or they descend into their dwellings, whence they are difficult to dislodge.* The burrows into which these owls have been seen to descend on the plains of the river Platte, where they are the most numerous, were evidently excavated by the marmot, whence it has been inferred by the learned and indefatigable Say*, that they were either common, though unfriendly residents of the same habitation, or that the owl was the sole occupant by right of conquest." We have in the statements of Captain Head, however, a proof that both tenants habitually resort at the same time to one burrow; and we are assured by Pike and others, that a common danger often drives them into the same excavation where lizards and rattlesnakes also enter for concealment and safety.

In the above extracts we have noted in italics the striking similarity to the account given by Captain Head. E. S.

19. *Naturalisation of Fish.*—We have received the following from Mr. Arnold of Guernsey.

Sir, 16th *August*, 1827.

Having understood that the correctness of Dr. Mac Culloch's statements respecting my pond, and the attempts to propagate sea fish in it, have been questioned, I beg to say that his statements are perfectly correct; and to add further, that during nearly four months of the year the water is perfectly fresh, and is drunk by cattle.

In summer, the saltness varies; but no examination yet made has discovered in it more than half as much salt as is contained in the neighbouring sea-water.

I further beg leave to add, that the general size of the pond in summer is about four acres and a half; in winter, when swelled by the rains, it is extended to upwards of fifteen acres; which will account for the freshness of the water.

I remain, Sir, your obedient humble servant,

To the Editor of the Quarterly Journal. J. B. Arnold.

20. *Mode of keeping Apples.*—It seems not to be generally known, that apples may be kept the whole year round by being

* We have had no opportunity of consulting Say, and therefore can only refer our readers to an author who has collected an interesting store of facts relative to natural science, and particularly with regard to this bird.

immersed in corn, which receives no injury from their contact. If the American apples were packed among grain, they would arrive here in much finer condition. In Portugal it is customary to have a small ledge in every apartment, (immediately under the cornice,) barely wide enough to hold an apple: in this way the ceilings are fringed with fruit, which are not easily got at without a ladder; while one glance of the eye serves to shew if any depredations have been committed.

21. *On the Cultivation and Forcing Sea Kale.*—The Crambe maritima, or Sea Kale, is an indigenous plant of this and other countries of Europe, and found on the sandy beach of the sea-shore.

It has been long introduced into our gardens as a culinary vegetable, but it is only within the last thirty years, that it has been brought into general use, and subjected to a mode of cultivation, very different from that which was first bestowed upon it.

The principal value of this plant is its property of early growth; appearing at table at a time when few such things can be had. It precedes asparagus, for which it is no bad substitute; and as it makes a dish of itself, it gives a variety to the delicacies of the table; and if the opinions given of its medicinal virtues be correct, it is well worth cultivation, and the notice we are about to take of it, in describing an easy method of having it in great perfection throughout the winter months, and up to the time it may be gathered from the natural ground.

Prepare one or more beds (with alleys two feet wide between) for the reception of the seeds, in the following manner: mark out the bed or beds two and a half feet wide, and of any required length, as near as can be from east to west; line off the sides and ends, driving a stake at each corner to ascertain the boundaries; dig out the earth of the bed one spade deep, removing it to some distance; fill this excavation with the purest and finest sand which can be procured in the neighbourhood, either from the sea-shore, the bed of a river, or from a pit. It signifies nothing of what colour it is, so it be pure, and as free from loam as it can be had; for in proportion as the soil of the bed is poor or rich, so will the flavour of the plant be when dressed. When this precaution is not taken, and when the plants are suffered to enjoy the rich and cultivated soil of a kitchen garden, or the situation made so, by rich dressings or coverings of fresh manure, the plants are stimulated into an unnatural luxuriance, which deteriorates the flavour, imparting to them that strong disagreeable scent and taste, resembling common cabbage, than which nothing can be a greater drawback on the value of the vegetable; but when grown entirely in pure sand, the flavour is mild and pleasant, and is relished by most palates.

When the bed is filled with sand and raised therewith about six inches above the natural level of the ground, (and this should be done previous to the end of March, which is the sowing season,) draw a drill along the middle, from end to end, about three inches

deep, in which drop the seeds pretty thickly, as they can be thinned out to the proper distance after they come up. If the sand or weather be dry at the time of sowing, give a little water in the drill and immediately cover up. If the seed be good, the plants will soon appear, and when they are advanced to a size large enough to enable the gardener to choose the most promising, let them be thinned out to the distance of six or seven inches, the distance at which they may remain. During the summer, the bed should be occasionally watered with *dung water;* and this for the purpose of encouraging the growth of the plants on their first setting off; and as manure given in this shape is more fugitive than when applied in a more solid or concentrated state, it cannot impart rankness to the plants when they arrive at that age fit to be brought to table.

The plants cannot be forced, nor should any of their shoots be cut, the first winter after sowing; but should be suffered and assisted to establish themselves, and gain sufficient strength to yield adequate crops, in the succeeding years.

About the month of November in the second winter after sowing, a part at one end of the bed should be prepared for forcing. For this purpose, and in order that it may be done with facility and effect, a rough wooden frame or frames should be made, eighteen inches high behind, and one foot high in front, shaped like a common hot-bed frame, and of any convenient and portable length; and in width, the same as the bed. Light wooden covers in convenient lengths should be fixed by hinges to the back; these may be raised at will for admission of light and air, and, in fine weather, may be thrown entirely back. When the frames are placed, dig out the alleys one foot deep to receive linings of hot dung, which may be banked up against both the back and front of the frame. The surface of the bed within the frame must be covered with soft, short straw, or hay, nine inches thick, to arrest the heat which rises from the linings, and form that warm humid region into which the shoots will advance. The temperature of these dark frames must be regulated by due attendance; and in intensely cold or frosty weather, the frames at night will require coverings of mats and litter, to prevent the plants receiving a check.

The required supply of the family—the time for it—and the length or number of the frames, must be judged of by the gardener, and who will act accordingly; but two frames are indispensable; because the second should be considerably advanced by the time the crop in the first is all cut.

Young plants may be transplanted; and if they are to be had, may be tried; but the safer way is to sow and plant both, to prevent disappointment; and in order that the roots be not too much exhausted by forcing, one bed should be forced in one year, and another the next.

The crowns of the roots have a tendency to rise; and as annual

additions of sand will be required after the autumnal dressing, the beds by these additions become unsightly; but cutting off the most aspiring (with its flowering stem) every summer, will keep the whole within proper bounds. Instead of covering with dung or litter, to protect from winter's frost, the frames may be set on those parts intended to be forced, to answer that purpose; and the uncovered parts of the beds may receive a coat of mould out of the alleys, to be drawn back off the sand in the spring.

The writer of this began to force Sea Kale as long ago as 1798, using hot dung within, as well as without, a frame with glazed lights; but soon found that, neither the glass nor dung *inside* was necessary or suitable: he, therefore, afterwards succeeded, by the above plan, to produce the finest crops of this vegetable, at any time in the winter months; and can confidently recommend such management, especially to those who have no hot-house or hot-bed frames; because when there is any early forced house or frames, if old roots are properly selected and potted in the autumn, and placed in such house or frame, where there is sufficient heat, and well shut up from light by whelming other empty pots over them, a crop may be had in this way, without the trouble and expense of out-door forcing.

J. M.

METEOROLOGICAL DIARY for the Months of September, October, and November, 1827, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For SEPTEMBER, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Saturday....	1	37	60	30.20	30.20	E	EbS
Sunday	2	42	61	30.20	30.18	NE	NE
Monday....	3	44	64	30.18	30.18	NE	NE
Tuesday ...	4	51	59	30.18	30.18	NE	NE
Wednesday.	5	51	61	30.17	30.17	NE	NE
Thursday ..	6	48	57	30.17	30.17	NE	NE
Friday	7	51	59½	30.17	30.15	NE	NE
Saturday...	8	52	60	30.11	30.07	NE	EbS
Sunday	9	51	62	29.83	29.77	EbS	SW
[illegible]	10	56	67	29.74	29.69	SW	SW
[illegible]	11	65	69½	29.66	29.59	SSE	SbW
[illegible]day.	19	55	62½	29.54	29.57	SW	W
[illegible]ay ..	13	50	61	29.70	29.93	W	W
[illegible]	14	46	65	30.03	30.03	W	W
[illegible]	15	50	67	30.10	30.13	W	WNW
[illegible]	16	58	67	30.17	30.17	WbN	W
[illegible]	17	57	66	30.17	30.16	E	ENE
[illegible]	18	57	62	30.13	30.10	ENE	NE
Wednesday.	19	45	57	30.02	30.02	SW	W
Thursday ..	20	46	54	29.69	29.70	SE	NE
Friday	21	43	63	29.70	29.48	NNE	SW
Saturday...	22	45	60	29.44	29.34	SW	WbS
Sunday	23	43	59	29.30	29.40	SW	WSW
Monday ...	24	40	60	29.50	29.50	SE	SW
Tuesday ...	25	42	61	29.30	29.50	SSW	S
Wednesday.	26	48	65	29.48	29.48	SE	S
Thursday ..	27	52	66	29.48	29.57	NE	NE
Friday	28	49	61	29.57	29.57	NE	SE
Saturday...	29	47	61	29.57	29.54	E	SE
Sunday	30	53	63	29.60	29.60	SE	SE

For OCTOBER, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest.	Morn.	Eve.	Morn.	Eve.
Monday....	1	52	65	29.67	29.70	SE	SE
Tuesday ...	2	54	63	29.74	29.90	E	ENE
Wednesday.	3	46	62½	30.04	30.12	ENE	ENE
Thursday ..	4	43	60	30.21	30.23	NE	NNE
Friday	5	42	62	30.28	30.17	NNE	NNE
Saturday...	6	44	61½	30.10	29.96	E	E
Sunday	7	43	58	29.87	29.73	E	SbE
Monday....	8	39	61	29.60	29.33	SE	SE
Tuesday ..	9	46	62	29.20	29.19	SE	WSW
Wednesday.	10	48	53	29.20	29.13	SW	NE
Thursday ..	11	46	56	29.04	29.08	SW	SW
Friday.....	12	44	55	29.12	29.32	W	W
Saturday...	13	34½	53	29.39	29.30	W	W
Sunday	14	44	57	29.60	29.68	W	SW
Monday....	15	46	61	29.70	29.71	SW	SSW
Tuesday ...	16	57	64	29.71	29.71	SW	SSW
Wednesday.	17	49	62	29.71	29.69	SE	SE
Thursday ..	19	38	61	29.69	29.63	SE	E
Friday	19	45	60	29.13	29.63	ENE	E
Saturday...	20	52	62	29.63	29.59	E	E
Sunday	21	50	59	29.50	29.42	E	E
Monday ...	22	53	59	29.23	29.10	EbS	SE
Tuesday ...	23	51	59	29.02	29.18	SE	SE
Wednesday.	24	47	60	29.44	29.74	SE	WbS
Thursday ..	25	46	59	29.87	29.38	WbS	SSW
Friday.....	26	52	61	29.96	29.84	SW	SE
Saturday...	27	46	60	29.63	29.46	SE	SSW
Sunday	28	45	57	29.31	29.50	NE	NE
Monday ...	29	33	48	29.77	29.88	NE	WNW
Tuesday ...	30	33	52	29.82	29.68	W	W
Wednesday.	31	46	53	29.64	29.64	NW	NNW

For NOVEMBER, 1827.

		Thermometer.		Barometer.		Wind.	
		Lowest	Highest	Morn	Eve.	Morn.	Eve.
Thursday ..	1	32	50	29.87	29.90	NW	NW
Friday	2	37	51	29.63	29.90	W	NNW
Saturday...	3	30	53	29.93	29.93	W	W
Sunday	4	41	55	30.09	30.03	W	W
Monday....	5	44	56	30.10	30.30	W	W
Tuesday ...	6	45	57½	30.19	30.11	W	W
Wednesday.	7	45	48	30.06	30.03	W	W
Thursday ..	8	43	49	30.00	29.96	E	E
Friday.....	9	44	53	29.77	29.68	W	WbN
Saturday...	10	46	54	29.73	29.80	WbN	W
Sunday	11	47	57	29.80	29.90	W	NW
Monday....	12	35	53	29.97	30.00	NW	WbN
Tuesday ...	13	40	60	30.00	30.02	WbN	WbN
Wednesday.	14	50	50	29.98	29.88	EbS	SE
Thursday ..	15	41	48	29.66	29.50	SW	SE
Friday.....	16	37	47½	29.30	29.30	E	ESE
Saturday...	17	30	47	29.62	29.69	ESE	SE
Sunday	18	36	51	29.86	29.95	E	E
Monday....	19	45	50	30.02	30.02	EbS	S
Tuesday ...	20	45	48	30.00	29.91	W	NW
Wednesday..	21	36	39	30.01	30.01	NbW	N
Thursday ..	22	31	34	29.80	29.60	W	W
Friday	23	17	36	29.58	29.47	W	W
Saturday...	24	22	33	29.60	29.79	WbN	WbN
Sunday	25	21	39	29.79	29.80	W	W
Monday....	26	37	45	30.09	30.12	WNW	NW
Tuesday ...	27	38	47	30.17	30.10	SW	SW
Wednesday.	28	36	45	29.88	29.70	S	SE
Thursday ..	29	39	49	29.27	29.30	SE	WbS
Friday.....	30	35	51	29.30	29.30	W	W

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